THURSDAY, APRIL 6, 1876

PREECE'S TELEGRAPHY

Telegraphy. By W. A. H. Preece, C.E., Divisional Engineer, Post Office Telegraphs; and J. Sivewright, M.A., Superintendent (Engineering Department) Post Office Telegraphs. (London: Longmans, Green and Co., 1876.)

IT is with feelings of great disappointment that we lay down the latest book on Telegraphy. In a work professedly a text-book of science, one of the series that contains Clerk Maxwell's Heat, Jenkin's Electricity, Goodeve's Mechanism, and the books of Bloxam and of Miller, we certainly were not prepared to find the part of science consistently left out. We considered, in fact, that when two authors, high in their profession, undertook to supplement a recognised want in so distinguished a series of text-books, we might expect to receive from them the best and the newest information obtainable on the subject that they profess to deal with.

In the last twenty years the advances in our knowledge of the science of electricity and magnetism and of the principles of telegraphy, a branch of electrical science applied, have been very great and very important. The science and the practical application of electricity have occupied a large part of the attention of the foremost investigators in physical science. The labours of Gauss, Weber, Faraday, Joule, Helmholtz, Thomson, Maxwell, and of the celebrated British Association Committee on Standards of Electric Resistance have given to the student on the one hand and to the practical engineer on the other, a system of laws, of methods of calculation, and of experimental results as to properties of matter, that make the science of electricity perhaps the most exact of all the branches of Natural Philosophy. To ignore every exact principle from first to last of a science so well founded, and to substitute for a clear statement of these well-known principles a few sketchy paragraphs not worthy of the middle of last century, is not, in our opinion, the proper introduction to a *scientific* text-book on Telegraphy for beginners.

The authors of the work before us naturally think that "Electricity being its main theme a certain acquaintance with the elementary principles of that science must be assumed on the part of the reader." However, "the differences which exist among electricians with respect to the signification of many of the technical terms employed in connection with telegraphy render it necessary that the student at the outset should have a clear comprehension of the meaning of those which will be used in this work." Accordingly they proceed to give him a "clear" idea of "Electric quantity," "electric potential," "electric resistance" in the following way:—

"Electricity is an agent pervading terrestrial and solar space, and is as universal in its effects as are heat and light. We are cognisant of its existence when we hear the roar of thunder and see the flash of lightning, but we do not know its particular form any more than we know that of heat or that of light. The sound of the thunder and the flash of the lightning affect the ear and the eye—we hear the sound and see the light—but we do not assume the existence either of sound or of light as distinc entities or things [Remarkable statement]. We can speak of the quantity of sound caused by the explosion of a cannon or by the blowing of a penny whistle, the quantity of light emitted by a gas jet or by a farthing rush-light; the quantity of heat required to melt a pailful of ice or to solder a metal joint, without implying by the term quantity a mass or volume of anything actually present."

Notions such as these as to quantity are quite new to us. We would suggest as an examination question for science classes the comparison of the quantity of sound caused by an explosion with the quantity caused by blowing a penny whistle for a week.

Our authors proceed:—

"The term implies relative magnitude only. It is the answer to the question, 'How much?' It implies the notion of more or less. When we speak of the magnitude of electricity present or passing, we speak of its *quantity*. When we read of the church spire destroyed, of trees riven to splinters, of wires fused, or of flocks killed, the damage done is due to the electricity passing, and the amount of that damage is proportional to its magnitude or *quantity*. If we take a piece of sealing-wax, a glass rod, or an ebonite comb, and rub it against the coat-sleeve, we find it has the property of attracting feathers, straws, and other light bodies. Electricity has been excited upon its surface, and the force of attraction is found to increase with the quantity of electricity present. Conversely the force with which bodies are attracted is an indication of the quantity of electricity excited. Hence we learn that ELECTRIC QUANTITY *is the magnitude or amount of electricity present.*"

[The italics and capitals in all the quotations are due to the authors.]

With no further explanation or hint of anything more definite, but with a few lines, apparently meant for an explanation of the algebraic method of representing numbers by letters, the authors calmly remark:—

"The unit quantity of electricity in general use has been called a *weber*, from Weber, one of the great German philosophers."

This is absolutely everything that is given to the elementary student to enable him to understand what is meant by quantity of electricity. Not a single experiment is described, except the electrostatic experiment of rubbing a glass rod, an experiment which has very little to do with the electromagnetic unit of quantity, the weber. Not a hint is given of the electromagnetic, electrochemical, or thermal effects of the current. We can picture to ourselves a student of lively imagination in sad perplexity as to whether the unit of current is to be expressed in terms of trees destroyed or of church-spires smashed per second, and as to the numerical relation between unit damage done and the weber. All this in the face of the beautifully simple absolute system of electrical measurement now universally adopted!

Space does not permit us to transfer to our pages in full the notions of the authors on other fundamental questions, though some of them are not less important than that which has just been noticed. A few lines of quotation must suffice.

"POTENTIAL," we learn, "*implies that function of electricity which determines its motion from one point to another.*"

We doubt whether the elementary student will obtain from this definition (?) or from the accompanying explanations, any criterion for knowing a point of high potential from a point of low potential, or any idea of how to reckon numerically the potential at a point, or even the difference of potentials between two points.

Next we learn that "RESISTANCE *implies that quality of a conductor in virtue of which it prevents more than a certain amount of work being done in a given time by a given electromotive force.*" This is prefaced by the extraordinary remark that "The transference of electricity, such as that from a charged cloud to the earth, from a rubbed glass to a rubbed comb, a signal from Europe to America, may take place in different *times;* the path between A and B offers obstruction to the passage of electricity; the medium through which it passes . . . is an obstacle to be overcome." The unit of resistance is, it is said, called the *Ohm*, from Ohm the German physicist, but the laws of Ohm are not stated, nor is the beginner even told to connect with them the idea of diminishing the current between two points by increasing the resistance between them; and with the exception of the mysterious and inaccurate reference to work done, just quoted, (and the work done in the circuit by no means presents itself directly to the attention of the beginner in electrical studies), there is nothing to afford a clue to the effect of the resistance in a given circuit. This remarkable chapter concludes as follows:—

"The nature of electricity itself is not known, nor is it necessary to the telegraphist that it should be known by him. He is only interested in its quantitative measurement and its application to practical purposes. Let him master its elementary principles, its general ideas, its properties and its conditions, and he can well afford to leave to physicists the discussion of its nature, and to mathematicians the determination of its laws."

We have no objection to these general sentiments, but we venture to think that for any one who wishes to write a book on telegraphy a moderate amount of attention to what the naturalist (we can't call him physicist) and mathematician have to impart would not be out of place.

We are far from satisfied with the chapter on batteries and with the chapters on construction; in fact our idea of what the artisan class and the beginners in science schools should expect to learn from a text-book of science in no way agrees with the ideas of the authors of this book. We cannot think that the last chapter (fifteen pages), describing the message-forms used in the British postal telegraphs, and a mass of technicalities even to the inserting of the received message in an envelope and handing it to the messenger, will be of use to anyone. And yet the dimensions allowed for the book have compelled the authors, as they tell us, "to abandon the submarine cable branch of the subject"! Similarly, "quadruplex, multiplex, and other novel systems of telegraphy have been omitted." . . . "The discussion of Ohm's laws and the apparatus depending upon them are not dealt with." Everything scientific is in fact left out.

There is a mass of technicality in the chapter on batteries, of which a host are described with their prices, &c. If space were of no consequence this would be most interesting but, for want of space, we presume, the principles of testing batteries for electromotive force and resistance are omitted. The chapters on construction contain elaborate discussions as to charring, tarring, burnetising, kyanising, boucherising poles, about the relative merits of different kinds of wood, about earth-borers, and a multitude of other things, while the principles of the parallelogram of forces practically applied to the fixing of poles by stays and struts are not mentioned. We should have thought that Mr. Preece, Mr. Sivewright, and their coadjutors leave but little choice to the artisan as to the materials and the tools to be employed in construction of telegraph lines, and that practical application of the science of dynamics to the case in hand would be of far higher value to the man who is actually engaged in stepping the poles and fixing the stays and struts, than a not very perfect description of the methods adopted for the preservation of timber. The gauge of the iron wire employed in Post Office Telegraphs is given. We are left to wonder whether in selecting wire any test as to electric resistance is applied or not; and we are not told what kind of preliminary electrical test, if any, ought to be applied. In fact, though these questions are intrinsically of extreme importance, they ought not to have been allowed utterly to displace the fundamental principles of the science of telegraph construction and of telegraphy. While they form a part of the whole subject, and they do form a very important part of it, they should have occupied a subordinate place in a book for beginners. Those who want an elementary text-book on the *science* of telegraphy, want more than a description of the practice, on their land lines, of the British Postal Telegraph department.

The chapters taken up with the instruments are, in our opinion, the best part of the book. The instruments used on land-lines are well described. Interesting information is given as to rates of working. Some of these show very wonderful results of practice. For example we learn that an experienced operator usually punches forty-five words per minute. Now a word contains 4·5 letters, and if we take it that an average letter contains, including the space that divides it from the next letter, four dots, we find that at this rate of punching 13·5 dots per second are made. If three more dots could be made per second, the strokes with the mallets would nearly cease to be heard distinct one from the other, and a deep musical note four octaves below the middle C on the pianoforte would be the result. We wonder whether this could be done were the operator to punch a few times over some sentence that he knows by heart.

We have noticed in reading the book some errors that it would be well to correct in future editions. On p. 21, damp ground is said to *abstract* a certain portion of the current. On p. 32, a battery is described as Thomson's, which is not used, and never was used, except for an experiment. Sir W. Thomson has arranged a gravity battery, very different in form from that here described, and which, being in use with his Siphon recorder, is generally known as his battery. In figures 69 and 93 the battery is represented as short-circuited by the sending key. On the whole, however, the diagrams are clear, and descriptions of the instruments well written.

MUSICAL INSTRUMENTS AT SOUTH KENSINGTON

A Descriptive Catalogue of the Musical Instruments in the South Kensington Museum. Preceded by an Essay on the History of Musical Instruments. By Carl Engel. Second Edition.

THE collection of musical instruments in the Museum at South Kensington is one of considerable and of varied interest. Consisting, as it does mainly, of the instruments employed by various nations within the last few centuries, it exemplifies the improvements in art and the gradual development of scientific principles in construction. But it includes also instruments of more remote dates such as would range within "the middle ages," and a few of prehistoric period. In the last case a similarity of musical instruments and of musical systems may be an important assistance in determining the ethnology of an extinct people, while the practice of opposite systems by neighbouring races will be a strong inference that they sprung from different stocks.

The excellent Catalogue, of which we have now a second edition, includes a prefatory essay by M. Carl Engel on the early history of musical instruments, and he has enhanced its utility by adding the scale of notes which many of the instruments would produce. This was practicable in the case of pipes, flutes, or other wind instruments from extant examples, but not with such as were stringed, because they would not remain in tune.

When a nation is found to have employed instruments constructed for the diatonic octave scale, such as the intervals of our A B C D E F G A, it is a proof that the possessors practised harmony, although it may have been but to a limited extent. The reason is that the fourth and seventh notes of such scales require different basses to the others, and would not be agreeable to the ears without the addition of such basses. On the other hand, if we find a scale of but five notes within an octave, the two omitted will be the fourth and seventh of the scale on that very account. We may in such cases conclude with safety that melody was the chief attraction to the people, and that harmony was either unknown or little appreciated.

We should bear in mind that the most ancient recorded *octave* scale of Greece and of Northern Asia and Africa is the minor; but there may have been exceptional cases of the use of the major, because the ancient Greeks had a five-note major as well as a five-note minor scale in their chromatic system. Nevertheless, it can have been but little practised by them in early ages because it was never admitted into their precepts. There is no Greek treatise upon music which includes the major scale; and yet the transition from one to the other was most easy, for whoever begins upon the third note, instead of the first, of any minor scale, will transform it into its "relative" major. These are the particular points to be borne in mind in estimating remote dates for musical instruments.

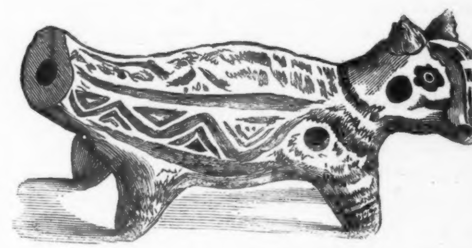


FIG. 1.—Ancient Pipe of the Chiriqui. (Centra America.)

And now to apply these principles to three of the instruments of different races discovered in Central and in South America. Among the ancient graves of the Chiriqui in Central America small pipes and whistles of pottery, which produce several sounds, have been found, of which one has six finger-holes, but in that case the

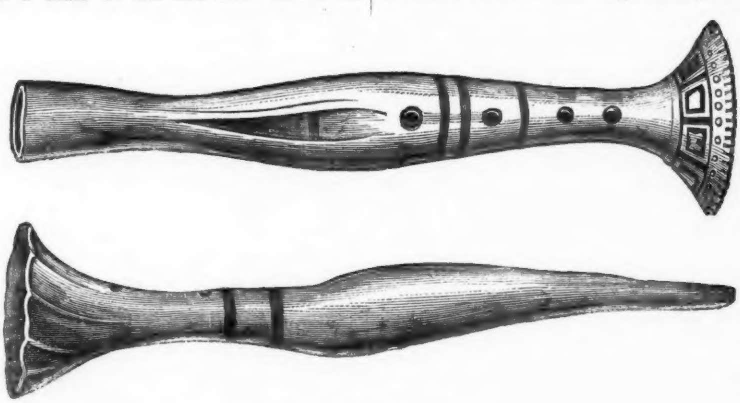
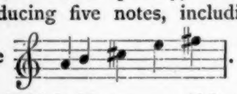


FIG. 2.—Pipes of the Aztecs.

notes have not been ascertained, while others have four notes, E F G A. These form the ancient Greek tetrachord, and the Greeks acquired their knowledge of music from the nations of Northern Asia and Northern Africa, especially from Egyptians, Chaldæans, and Phœnicians. Can any one of these nations have been in contact with the Chiriqui of Central America? The scale is fitted for chanted recitations rather than sufficient for music proper. F would be the chanting note of the above, with the power of ascending, for expression, to G and A, and with that of descending to the major seventh, E in preparing for a close. In this sense the notes are pure, good, and

true parts of a major scale. They are such as a correct musical ear would dictate, but they tell little as to the cultivation of music.

Upon the same page (67) M. Engel passes on to the *Pitos* (of the flageolet kind) used by the Aztecs. These are of reddish pottery, having four finger-holes, and producing five notes, including the open note of the pipe . Several examples are in the British Museum. Although Pitos vary in size they have all the same pitch of sound, says M. Engel, and are easy to blow. They have but five notes to the octave, in the major scale, and this has been popularly called "The Scotch Scale." M. Engel terms it *pentatonic*, but, if it must needs have a Greek name, we prefer *pentaphōnic*, since two of the intervals for an octave scale are of minor thirds. The points to be remarked in these Pitos are that the various lengths should have been so proportioned as to be of the same pitch, and that the scale should be major. We may say of the Aztecs, as of the Chiriqui, that the practice of harmony did not obtain among them, but the Aztecs had advanced further in the direction of melody, and had in all probability a custom of playing many pipes together in unison. They were also advanced in the manufacture of pottery. It is often convenient to suppose that a people is "indigenous," and it may have been so for many centuries, but the speculation for ethnologists is "Whence originally came the Aztecs?"

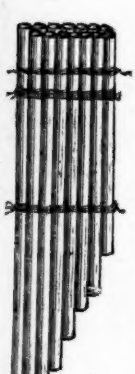
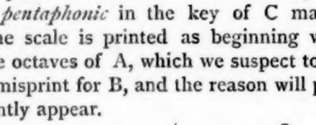
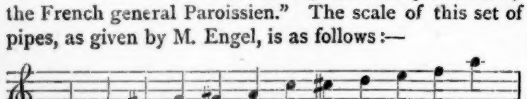


FIG. 3.—Huayra-puhura of the Inca Peruvians.

The third case is of the Inca Peruvians (p. 70). They had a Syrinx, or Pandean pipe, which played in octaves. The seven double pipes were of equal length, but the one pipe was closed at the end, and its fellow open. When M. Engel says: "The reader is probably aware that the closing of the pipe at the end *raises* its pitch an octave," it is an error in writing—he means that closing the end *lowers* its pitch an octave. The Peruvian Syrinx is *pentaphonic* in the key of C major. The scale is printed as beginning with the octaves of A, which we suspect to be a misprint for B, and the reason will presently appear.



Another instrument of the same class "was discovered placed over a corpse in a Peruvian tomb, and was procured by the French general Paroissien." The scale of this set of pipes, as given by M. Engel, is as follows:—



It commences with E, the major Seventh below F, the key-note, just as we suppose the preceding pipes should have been given. It then forms an ascending chromatic scale of F to the extent of an octave, excluding only the Fourth and the *minor* Seventh of the scale. The reason is evidently because those two are not good melodic intervals. They require their own basses to render them agreeable to the ear, and the instrument does not include

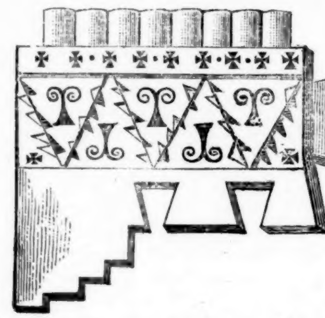
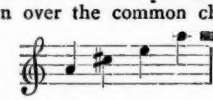


FIG. 4.—Huayra-puhura of the Inca Peruvians.

a bass. On the other hand the *major* Seventh, or semitone below the key-note, is included. It is a good melodic interval, and the French term it *la note sensible.* The only tone above the octave is A, which adds to the instrument a run over the common chord of A major.

.

This is a remarkable and an excellent melodic scale, which has not been duly appreciated by M. Engel. He says: "The musician is likely to speculate what could have induced the Peruvians to adopt so strange a series of intervals—indeed it seems rather arbitrary than premeditated."

A little reflection upon the intervals of the harmonic, or natural, scale would have satisfied M. Engel that these Peruvians had exceptionally correct ears for music. It is not, as he supposes, any "natural predilection for that series of intervals which may be called the pentatonic scale, *because* it contains only five different intervals in the compass of an octave" (p. 311) but, "because" the two omitted notes are unsatisfactory to the ear in unaccompanied melody. They are "irrational" intervals, and therefore they have been rejected by nations in all quarters of the globe, Scotch and Irish, Egyptians, Chinese, Malays, Peruvians, Aztecs, and others. Even Europeans, who cultivate harmony, reject the minor seventh in minor scales.

M. Engel has fulfilled a laborious duty with great industry and ability. A more intricate subject than the ever-changing names of the musical instruments of past ages could scarcely be found. It is next to impossible that it should be quite perfect. Sometimes the names of instruments were changed without any appreciable reason, and, in many cases, those names were absurdly misplaced. For instance, an Italian monochord for measuring intervals was called *tromba marina*, and was translated into English as a "trumpet marine." Dr. Burney not unnaturally assumed that it must have been a trumpet made of a shell, like the Triton's conch, but it consisted of three long pieces of wood, glued together triangularly and it had one string to be sounded by a bow. Again, as to the guitar in Spain. It had but four strings until about the sixteenth century, and then, seeing that foreign

viols had six strings, the Spaniards increased the number upon their guitars to six, and termed them vihuelas, without adopting the distinctive feature of the viol—the bow. "No es otra cosa esta guitarra sino una vihuela quitada la sexta y la prima cuerda," says Juan Bermudo, in 1555,—"This guitar is no other than a vihuela deprived of its first and sixth string"—but M. Engel has, by mistake, given the name of vihuela to the earliest guitars with four strings, as upon the portico of the church of Sant-Jago di Compostella, and others. In England, in the sixteenth century, the vihuelas were termed Spanish viols, and are so named among the musical instruments left by Henry VIII., but eventually they were reinstated in the name of *Spanish* guitars, while the old English cittern, strung with wire, and the number of strings increased from four to six (single or double), was *the* guitar of the last century. M. Engel has not recognised this change. The English had also a small instrument with four catgut strings, very like the four-stringed Spanish guitar called the gittern, or ghittern. It differed chiefly from the Spanish instrument in having a lute-shaped back instead of a flat one. M. Engel does not explain how this instrument differed from the guitar, although he has given to another its Latinised name, quinterna or guinterna.

Again, it is not in accordance with English usage to term large pieces of bamboo partially split, in order that, by shaking them, the sides may be rattled together, "castanets;" neither were castanets called *crotala* by the ancients, as M. Engel supposes. The ancient castanets were made of nut-shells, cockles, oyster-shells, or small pieces of metal, and were called *krembala*. "And beating down the limpets from the rocks," says Hermippus, "they made a noise like castanets" (κρεμβαλίζουσι). The *krotala* were maces and other large and loud rattles to be used in the worship of Cybele. Sometimes the two parts were detached and held in two hands, and sometimes they had a hinge or spring at one end, to be sounded by closing the hand suddenly so as to knock one against the other. The stork was called *crotalistria*, from the noise made by the bird in striking together the two bones of its beak.

M. Engel's history is a little at fault when he writes that "The earliest organs had only about a dozen pipes," and that "The largest, which were made about 900 years ago, had only three octaves, in which the chromatic intervals did not appear" (p. 108). This passage must be looked upon only as a prelude, intended to magnify, by contrast, the improvements which have been made in Germany and other parts of the Continent of late years. It can have no other meaning—for the organ at Winchester had 400 pipes in the tenth and eleventh centuries. It is fully described by a contemporary musician, the author of a once celebrated, but now lost, treatise, *De Tonorum Harmonia*. Two lines of extract will suffice:—

> "Sola quadringentas quae sustinet ordine musas,
> Quas manus organici temperat ingenii."
>
> Wolstan's *Life of St. Swithun.*

M. Engel makes also a mistake of from 400 to 500 years in the date of a manuscript, and, acting upon this mistake, he claims priority for Germany over England for the first use of the fiddle-bow. Strings have but a fleeting tone, unless it be sustained by friction, and the principle is so simple as to be intelligible at one glance, and therefore to be readily adopted. But the first example yet discovered is in an Anglo-Saxon MS. of the earlier half of the eleventh century, where one gleeman is playing the fiddle while another is throwing up and catching three balls in rotation (Cotton MS, Tiberius, c. vi.). This is the figure with the primitive fiddle:—

FIG. 5.—Anglo Saxon Fiddle. XIth Century. (British Museum.)

It will be seen that the shape of the body of the instrument prevents the use of the finger-board for the production of the higher notes, and to obviate this difficulty, a hole was subsequently made through the back, so that the performer's hand might have more command over the strings. In this form the Anglo-Saxon or early English name was *cruth*, Anglicè, *crowd*, a crowder and a fiddler being synonymous.

> "The fiddler's crowd now squeaks aloud," &c.

FIG. 6.—Crowd. English. About the XIIIth Century.

The third stage of improvement was to diminish the size of the body, to give it indented sides so as to allow free action to the bow, and lengthen the neck, as in the

early English *citra*, *cetera*, or more modern *cittern.* "Fu la cetera usata prima tra gli Inglesi," says Galilei. Examples of instruments of this kind are frequent, but M. Engel startles us by exhibiting the following as an original "*German* Fiddle, IXth century, St. Blasius."

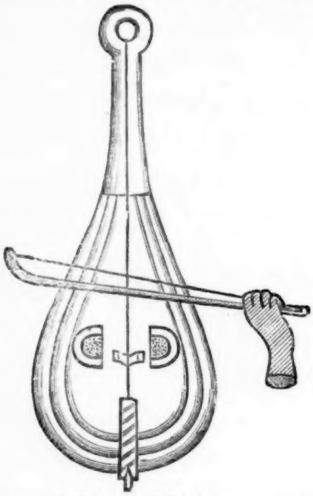


FIG. 7.—Fiddle String. XIIIth Century. (St. Blaise.)

The above is copied from Gerbert's "De Cantu et Musica Ecclesiæ," vol. ii., and is derived from a manuscript which was formerly in the monastery of St. Blaise, and which Gerbert describes as "about 500 years old!" (ex cod. San. Blas. annorum. cirç. 500, p. 139) as he wrote in 1774. That would bring the date to the second half of the *thirteenth* century, instead of the eighth or ninth, as M. Engel states in his text. Moreover the plate is not intended to exhibit a fiddle, but a fiddle-*string*. It is called *lyra*, and the word is explained in two of the glossaries collected by Mr. T. Wright. A fiddle would have had more strings than one, in the thirteenth century, and the strings would have been fastened to pegs instead of a single string passed through a ring.

It will be seen from the above that the claims for Germany are put forth in the strongest light by M. Engel, and that other countries may not so readily acquiesce in them. We ourselves should raise many demurrers to his claims and conclusions, but they would apply to the prefatory essay and to the musical instruments of Europe, rather than to those of the rest of the world. While we cannot but wish that M. Engel's nationality had been less strongly developed, he is justly entitled to the credit of having ably fulfilled his commission, and of having exerted extensive research.

OUR BOOK SHELF

Notes on the Practical Chemistry of the Non-Metallic Elements and their Compounds. By William Procter, M.D., F.C.S., Lecturer on Chemistry at St. Peter's School, York. (London: Simpkin, Marshall, and Co. York: the Northern Educational Trading Co.)

THIS is a handbook on the Practical Chemistry of the Non-metallic Elements, designed to meet the requirements of pupils of Mechanics' Institutions, and of Science Classes of a similar kind. The true man of science welcomes every worthy means of spreading scientific truth, and does everything in his power to propagate that truth. He will regard with a jealous eye each work brought forward with a view of extending a knowledge of the sciences; and with a work intended for the use of a class whose opportunities of gaining knowledge are very limited, his scrutiny will be all the closer. A book written for the information of such should be couched in the simplest language, and the sense conveyed should be at once clear and comprehensive. In these respects Dr. Procter's little work cannot be termed a success. To use no stronger expression his language is frequently very vague. For example, on page 14 the author in speaking of "Chemical Affinity" says: "hence, in order that this force may be exercised by the particles coming within the sphere of each others' attraction, the substances must be in the state of liquid or gas." There can be but one way of understanding the latter part of this quotation, viz., that no chemical action can take place, unless the materials taking part in that action are each and all of them in a liquid or gaseous state. Dr. Procter is scarcely less happy in his definitions of bases, acids, &c. He says: "An acid is a compound of an electro-negative radical with hydrogen, which hydrogen it can exchange for a metal or basylous radical, and it is therefore replacable." Again, "A salt is a compound produced by the action of a base upon an acid with the displacement of the hydrogen of the latter." How can such definitions convey to the minds of pupils proper ideas of the true natures of acids and bases? Such explanations would not inappropriately be termed *indefinitions.* Chapters are devoted to chemical calculations, and chemical manipulations, and here doubtless the readers will find many useful hints for their guidance in the preparation of their apparatus, &c. In the body of the book Dr. Procter treats of the non-metallic elements, giving the ordinary methods of preparing them, and their compounds, and illustrating the characteristics of each by interesting and instructive experiments. A few pages devoted to the chemistry of water, qualitative analysis of gases, and the preparation of ordinary reagents, complete a book, which, designed for a good purpose, and containing much useful information, at the same time shows want of care in compilation, and also lacks lucidity. Printer's errors are much too numerous.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

The Use of the Words "Weight" and "Mass"

Nothing could illustrate more forcibly the necessity of rendering definite the meaning of some of our present fundamental terms in connection with the science of dynamics, than a comparison of my letter to you on this subject (vol. xiii. p. 325) with the letter of Mr. Stoney, in reply to it (vol. xiii. p. 385), and that of Mr. Walker who follows Mr. Stoney.

When we who call ourselves teachers do not agree as to the signification of the most elementary terms we use, it is not to be wondered at, if those who come to learn should fail to attain clear ideas on a part of the science, where such confusion of nomenclature prevails.

My letter to you was for the purpose of pointing out an ambiguity of language, and of suggesting, in order to get rid of this ambiguity, not an alteration of the meaning of any word whatever, but a very simple *restriction* in the use of words, and the bringing into more frequent employment a very valuable old word—*gravity*—which has been lying ready for use but left almost idle. Mr. Stoney admits the ambiguity to be a very real one.

Mr. Stoney, however, says: "I fear Mr. Bottomley's remedy,

f adopted, would introduce quite as serious, perhaps a more serious ambiguity. Gravity is an acceleration. When we say that gravity is less in a balloon, or in a mine, than at the surface of the earth, or greater at Glasgow than at Manchester, we are speaking of alterations of g—the acceleration due to the earth's attraction; and it would create confusion to employ this word to designate forces also."

Now I do not think that the use of the word gravity as an acceleration is at all common. On the contrary, I have looked into all the books at my command and I cannot find any support whatever for such a use of the word. Every one is accustomed to speak of the "force of gravity." To speak of the force of an acceleration would be a complete anomaly.

All the dictionaries that I have seen support my view that gravity means force, and does not mean acceleration. Take, for example, a good modern book, the Imperial Dictionary. There I find—Gravity (Lat. *gravitas* from *gravis*, heavy). 1. Weight, heaviness. 2. In philosophy—that force by which bodies tend or are pressed, or drawn towards the centre of the earth—and so on; showing that gravity always means force, though it has various shades of meaning in its occasional applications.

Chambers' Encyclopedia says that the *force* which causes bodies to fall towards the earth is termed *gravity*. The article goes on to speak of the force of gravity at the earth, at various heights above the earth, and at the moon. Acceleration is nowhere mentioned as a meaning of the word gravity.

Even if it could be shown that a few people have so far departed from the original etymological sense and prevailing use of the word gravity, as to employ it for an acceleration instead of for a force, this would only prove that the word is, to that limited extent, subject to ambiguity at present. The course that I advocated was to avoid ambiguity by employing the word *gravity*, used in its most common, and most authoritative sense, instead of a thoroughly ambiguous word *weight*, in certain cases where misapprehension from the use of the latter word is likely to occur.

It seems to me that Mr. Stoney and Mr. Walker have been led away by thinking of the letter g, rather than of the important question at issue. The letter g stands for a *number*. One way of specifying what number g stands for, is to say that it is the numerical expression for the acceleration *due to gravity*, that is, the acceleration due to the force of gravity acting on a falling mass. But this is simply because g is the number which represents the force in Gaussian units on unit mass, and because the force of attraction on any body is proportional to the mass of the body. *The latter is an experimentally discovered law.* To say with Mr. Walker that "the symbol 'g' is gravity," and with Mr. Stoney that "gravity is an acceleration," seem to me expressions equally elliptic on the one hand, and, without full explanation, misleading on the other.

If Mr. Stoney offers any method of getting rid of the ambiguity better than that already introduced, others will gladly adopt it. As to Mr. Walker's proposal to confine the use of the word "weight" to mean force, my former letter was partly for the purpose of showing this to be impossible. The act of parliament, regulating weights and measures, settles that matter. Mr. Stoney's letter forms, also, a sufficient answer to Mr. Walker's proposal.

University, Glasgow, March 27 J. T. BOTTOMLEY

P.S.—Allow me to thank Mr. Barrett for his information as to the earliest use of spring balances for kilodynes.

If Mr. Walker is serious in proposing to use *vires* for British kinetic units of force, he ought to avoid *centivires* and *millivires* for 100 vires and 1,000 vires respectively. These would be utterly incompatible with the use of the prefixes *centi*, *hekto* and *milli*, *kilo* in the now established metrical system.

Birds as Astronomical Objects

THE following note which appears in the last number of *Stray Feathers* (iii. p. 419), seems to deserve more attention from astronomers than it will perhaps receive unless published where it will meet the eyes of others than Indian ornithologists. I beg leave, therefore, to ask that it may be reproduced in NATURE.

"Looking at the sun this morning, I saw birds very frequently pass the disc. Some were in focus with the sun itself, the wings being quite sharp against the disc, and must have been several miles high, but some were much nearer, and I estimate their distance from me at about two miles by the focus required to see them. These last must, however, have been quite a mile above the earth's surface, and of course many were a great deal higher.

"I suppose they were Kites, but the appearance there was rather as though the wings were long and narrow like those of Swallows, whereas I should have expected the points to be blunted by the irradiation.

"The estimated distance between the tips might be a couple of feet.

"Possibly this may interest some of the readers of *Stray Feathers*. "J. TENNANT, R.E.

"Roorkee, 23rd September, 1875."

On this note the editor of *Stray Feathers*, Mr. A. O. Hume, remarks:—

"Many of those birds must have been quite invisible to the naked eye. I have no doubt that Vultures, Kites, and Eagles often soar for hours at heights at which they are thus invisible to us, though we and our doings are quite within the grasp of their far-seeing gaze. This would help to account for the marvellous manner in which, when an animal is killed in the plains, an apparently speckless sky becomes in an incredibly short space of time crowded with 'an heavenly host.'"

We know so little with respect to the height at which birds do or can fly, that I am sure all ornithologists would gladly avail themselves of any observations on the part of helioscopists or other astronomers that would bear upon the matter, and I may add that perhaps the evidence they could offer might be of importance as regards the migration of birds. In Mr. Hume's remarks I entirely concur. ALFRED NEWTON

Magdalene College, Cambridge, March 25

How Typhoid Fever is Spread

THE case in which the poison of typhoid fever mixed with drinking water was transmitted through nearly a mile of porous earth, and which was mentioned in the abstract of my discourse to the Fellows of the Chemical Society (NATURE, vol. xiii., p. 331), is fully described (in German) in the 6th Report of the Rivers' Commission on the Domestic Water Supply of Great Britain. It will shortly appear, in English, in the Monthly Journal of the Chemical Society. Meanwhile perhaps I may be allowed to trespass upon your space with the following remarks:—The outbreak of typhoid fever occurred at the village of Lausen, near Basel, in Switzerland, and it was exhaustively investigated by Dr. A. Hägler of Basel, who has given a full account of it in the "Deutsches Archiv. f. Klin. Med. xi." The source of the poison was traced to an isolated farmhouse on the opposite side of a mountain ridge, where an imported case of typhoid, followed by two others, occurred shortly before the outbreak. A brook which ran past this house received the dejections of the patients and their linen was washed in it. This brook was employed for the irrigation of some meadows near the farm-house, and the effluent water filtered through the intervening mountain to a spring used in all the houses of Lausen, except six which were supplied with water from private wells. In these six houses no case of fever occurred, but scarcely one of the others escaped. No less than 130 people, or seventeen per cent. of the whole population, were attacked, besides fourteen children, who received the infection whilst at home for their holidays, and afterwards sickened on their return to school.

The passage of water from the irrigated meadows to the spring at Lausen was proved by dissolving in it, at the meadows, 18 cwt. of common salt, and then observing the rapid increase of chlorine in the spring water; but the most important and interesting experiment consisted in mixing uniformly with the water 50 cwt. of flour, not a trace of which made its way to the spring, thus showing that the water was *filtered* through the intervening earth and did not pass by an underground channel.

These are the main features of the case, but there are other interesting details showing how carefully the investigation was conducted; for these, however, I must refer Mr. Mitchell Wilson to the works above cited. It affords a clear warning of the risk which attends the use, for dietetic purposes, of water to which even so-called *purified* sewage gains access; notwithstanding that, as at Lausen, such water may have been used with impunity for years, until the moment when the sewage became infected with typhoid poison.

E. FRANKLAND

Lisbon Magnetic Observations

MR. DE BRITO CAPELLO, Director of the Lisbon Magnetic Observatory, having addressed to me several interesting results having reference to the notice of his observations which appeared in NATURE, vol. xiii. p. 301, I am anxious to communicate them to your readers.

With reference to the movement of the declination magnet from 8 A.M. to 2 P.M. Mr. Capello gives me the following mean values for each year from 1858 to 1875:—

1858	8′·74	1867	6′·15
1859	10·54	1868	7·17
1860	10·11	1869	8·42
1861	9·00	1870	10·83
1862	7·84	1871	10·60
1863	7·65	1872	9·45
1864	6·94	1873	8·22
1865	6·61	1874	7·23
1866	6·19	1875	6·09

These quantities, Mr. Capello remarks, show the maxima 1859·8 and 1870·9, and the minimum 1867·1, agreeing very nearly with the epochs of maximum and minimum sun-spots.

It also appears as if the minimum had been reached again last year, the mean oscillation (6′·09) being less than in 1867. This agrees with the conclusion derived by me from the Trevandrum observations, and communicated to the French Academy of Sciences last year. Dr. R. Wolf had previously (as I now find [1]) concluded from his sun-spot observations that the minimum would probably appear in 1875–6; and he considers we have now one of the short periods, which his tables of sun-spots show may be expected every 80–90 years. My own conclusion to the same effect (that we have now a short period) was founded on a consideration of the magnetic observations. The last short period was that from the maximum 1829·7 (shown by Arago's observations) to that of 1837·5 (shown by Gauss's observations), an interval of rather less than eight years. If we may take 1875·5 as the epoch of the present minimum, then the interval from the last is nearly nine years. As the interval from the minimum of Arago (1824·2) to the next was nearly 9·2 years, we find a space of nearly forty-two years from the last short period to this one. Should this hold for the next maximum it will occur about 1879·0.

Mr. Capello has obtained the interesting result that the curve showing the mean diurnal disturbance of the vertical magnetic force is the exact inverse of that for the mean diurnal disturbance of declination at Lisbon; a movement of the north pole of the declination magnet towards the west corresponding to one downwards of the south pole of the balance needle. It appears also that the difference of sign in the temperature coefficient for the balance magnet due to changes of the compensation bar from brass to zinc and zinc to brass, on which a remark was made in NATURE, vol. xiii. p. 302, does not affect the results for the diurnal variation, each year giving the same mean law of a minimum vertical force between 11 A.M. and noon, and a maximum near 5 P.M. whatever the sign of the temperature coefficient. It appears also that the results at Lisbon are confirmed by those obtained at Coimbra, ninety miles to the north.

JOHN ALLAN BROUN

The Early History of Continued Fractions

THE reviewer in NATURE, (vol. xiii., p. 304), very properly points out that the first mathematician who used continued fractions was Cataldi, and not Lord Brounker, as is still often stated.

To this fact I drew attention in a pamphlet, published in 1874, not then knowing that De Morgan had done the same many years ago. There is, however, in connection with the same subject, another historical fact almost equally interesting, which few in this country seem to be aware of, and which therefore it may be desirable to bring before your readers.

Daniel Schwenter, a professor at Altdorf, in the first quarter of the seventeenth century, made use of the present well-known process for expressing the ratio of two integers as a continued fraction, and calculated the convergent, exactly in the mode at present followed. He does not indeed seem to have written as we now do the actual continued fraction obtained in any case, but the process of repeated division, and the mode of finding the convergents were most fully described and exemplified by him.[1]

The following, therefore, seems to be, in few words, the early history of continued fractions:—

1. Cataldi published, in 1613, his discovery that the square root of an integer can be expressed as an interminate continued fraction, *e.g.*,

$$\sqrt{18} = 4 + \cfrac{2}{8 + \cfrac{2}{8 + \cfrac{2}{8 + \dots}}}$$

2. Schwenter, almost certainly without knowledge of what Cataldi had done, published in 1636 the mode of changing an ordinary fraction into a continued fraction with unit-numerators, and of calculating therefrom convergents to the given fraction, *e.g.*,

$$\frac{117}{233} = \cfrac{1}{1 + \cfrac{1}{3 + \cfrac{1}{6 + \cfrac{1}{4 + \cfrac{1}{2}}}}};$$

3. Brounker, very probably in ignorance of what had been done by Cataldi and Schwenter, made the discovery that

$$\frac{\pi}{2} = \cfrac{1}{1 + \cfrac{1}{2 + \cfrac{4}{2 + \cfrac{9}{2 + \dots}}}}$$

which was published by Wallis in 1655 ("Arithmetica Infinitorum" p. 181), along with a tolerably complete theory of continued fractions in general.

The necessary details bearing on these three main facts will be found in a painstaking work by Prof. Favaro, "Notizie Storiche sulle Frazioni Continue," Roma, 1875, or in shorter form, in a school "programme" by Dr. Günther, "Beiträge zur Erfindungsgeschichte der Kettenbrücke," 1872.

That Cataldi, Schwenter, and Brounker, starting from totally different points should all light on the continued fraction form, and that it should be twice (perhaps nearly thrice) lost, are certainly strange facts, forming a curious chapter in the history of scientific discovery.

THOMAS MUIR

High School of Glasgow

The Dry River-beds of the Riviera

MR. R. E. BARTLETT (NATURE, vol. xiii. p. 406) asks for some theory to account for the existence of the broad stony riverbeds of Piedmont. He instances the Paglione at Nice, which is indeed the merest rudiment of a river for the greater part of the year. But if Mr. Bartlett will wait, not so much for the snows on the Maritime Alps to be melted, as for the rainy weeks of autumn to come again,

> Quum fera diluvies quietos
> Irritat amnes,

he will see that that now despicable river annually flows with a vehemence and a volume worthy of its size. Many a dry and insignificant torrent-bed in the neighbourhood of Nice swells during the rainy season to a torrent indeed; the thoroughness with which they then drain the adjacent slopes is amply sufficient to explain their existence and their appearance when their "occupation's gone."

HENRY T. WHARTON

London, April 2

The Flame of Common Salt

HAVING been much interested in the progress of the investigations concerning the blue flame of common salt when thrown into a coal fire, I made the following experiments, by which I came to the conclusion that the origin of the blue flame is due to the presence of copper, which occurs in nearly every coal as an ingredient of the pyrites.

According to "Bercelius," by agency of the blow-pipe, small

[1] "Astronomische Mittheilungen," 38, p. 378, July 1875.

[1] See "Daniel Schwenter, Deliciæ physico-mathematicæ," Nürnberg, 1636, p. 111. "M. Daniel Schwenter's Geometriæ practicæ novæ et auctæ, Libri IV." durch Georgium Andrem Böcklern, Nürnberg, 1657, p. 431.

traces of chlorine can be discovered by dissolving at first a larger quantity of oxide of copper in a bead of microcosmic salt; if, then, any substance containing chlorine be brought in contact with the bead on the point of the blue jet, the blue flame of the blow-pipe is coloured azure blue; the same colour is even exhibited when chloride of sodium and oxide of copper are heated in the bead; the yellow flame of the sodium is scarcely visible, and does not conceal the azure blue flame.

In making the above experiment, I immediately suspected that the blue light imparted to the coal corresponds with the light of the microcosmic bead, the chlorine of the salt combines with the copper, which burns with the azure blue flame.

To convince myself that the blue colour is not due to any other cause, I took a charcoal fire free of copper, and threw salt into it; the salt gave a slightly yellow flame of sodium, and not the minutest trace of a blue flame could be discovered, though large quantities of salt were used, but on throwing the slightest quantity of oxide of copper on to the charcoal, the azure blue colour occurred instantly.

The following chlorides, HCl, KCl, NH_4Cl, $BaCl_2$, $CaCl_2$ which I had at my disposal, exhibited more or less a blue colour when cast into a coal fire. T. N. MÜLLER

Middleton St. George

OUR ASTRONOMICAL COLUMN

DOUBLE STARS.—Baron Dembowski communicates to the *Astronomische Nachrichten*, Nos. 2,076–2081, his later measures of double-stars, amongst which is a considerable number of well known binaries. The measures are given in a slightly modified form from that adopted in most of the Baron's previous important communications, which commenced in vol. 62 of the above periodical.

For the sake of facilitating a comparison with the best-determined orbits of revolving double-stars, we extract a few of the most recent Gallarate epochs or means of the year's measures:—

Name of Star.	Year.	Position.	Distance.	Position (*c* – *o*).	Distance (*c* – *o*).
		°	″	°	″
Castor ...	1875·25	235·03	5·54	+ 0·94	+ 0·39
ζ Cancri ...	1875·14	130·94	0·74	+ 0·88	– 0·05
ξ Ursæ Maj. ...	1875·27	317·56	1·09	+ 6·44	– 0·13
γ Virginis...	1874·93	159·35	4·77	+ 0·80	0·00
ξ Bootis ...	1874·89	286·32	4·54	+ 1·12	+ 1·02
η Coronæ Bor. ...	1875·41	66·69	0·86	– 3·13	– 0·13
ξ Scorpii ...	1874·96	68·97	7·12	– 1·80	+ 0·06
ζ Herculis ...	1875·52	149·09	1·41	– 1·20	– 0·09
70 Ophiuchi ...	1875·52	83·72	3·48	+ 3·11	+ 0·85
δ Cygni ...	1875·02	335·26	1·60	– 1·13	– 0·14
Σ 3062 ...	1875·67	292·17	1·47	– 4·62	+ 0·14

The orbits with which the above comparisons have been made are by the following calculators; the periods of revolution are added.

Castor ...	Thiele ...	996·85 years.
ζ Cancri ...	O. Struve ...	62·4 „
ξ Ursæ Majoris ...	Hind ...	60·68 „
γ Virginis ...	Thiele ...	185·01 „
ξ Bootis ...	Hind ...	168·9 „
η Coronæ Bor. ...	Wijkander ...	41·58 „
ξ Scorpii ...	Thiele ...	49·05 „
ζ Herculis ...	Dunér ...	34·22 „
70 Ophiuchi ...	Schur ...	94·37 „
δ Cygni ...	Behrmann ...	415·1 „
Σ 3062... ...	Schur ...	112·64 „

Comparisons are omitted in the cases of several binaries, the orbits of which have been worked up to about present time, principally by Dr. Doberck, of Markree Observatory. Mr. W. Plummer's orbit of ζ Cancri gives the angle 15° greater than the measures, the distance nearly agreeing. The orbit of ξ Ursæ Majoris by Dr. Ball, of Dunsink Observatory, Dublin, is not available as printed in the "Monthly Notices" of the Royal Astronomical Society, the excentricity being omitted, and the same element has escaped also in Dr. Doberck's orbit of σ Coronæ Borealis.

THE EQUATOR OF MARS.—Sir W. Herschel's determination of the position of the plane of the equator of this planet, from observations made in the autumn of 1783, was communicated to the Royal Society in December of the same year. He found for the inclination of the axis to the orbit, 61° 18′, and therefore for the obliquity on the globe of Mars, 28° 42′; the inclination of the axis to the ecliptic, 59° 42′, the north pole being directed in 1783 to longitude 347° 47′. From these figures we deduce for the ascending node of the equator of Mars upon that of the earth (N) 48° 9′, and its inclination thereto (I) 41° 27′.

Olbers, from the observations of Schroeter and Harding, on the south polar spot upon Mars in October and November, 1798, found for the longitude of this pole 172° 54′·7, and the latitude 60° 33′·2, and hence we have for the ascending node of the equator on the orbit, 84° 54′, and for the obliquity on Mars, 27° 57′; the ascending node of the equator of Mars upon the terrestrial equator 50° 29′, and the inclination 39° 14′.

But the determination which has been generally relied upon as the best yet available is that made by Dr. Oudemanns, now director of the Observatory at Batavia, from measures by Bessel with the Königsberg heliometer, 1833–37. For about 1834·0 he found the longitude of the north pole of Mars 349° 1′, and its latitude 61° 9′. The node of the orbit of the planet in the ecliptic being at this time in 48° 16′, and the inclination 1° 51′, we have for the ascending node of the equator of Mars on his orbit, 80° 50′, and for the obliquity of the Martial ecliptic, 27° 16′, and therefore for the ascending node of the equator of Mars upon the terrestrial equator, 47° 34′, and its inclination, 39° 56′. Assuming these figures to apply to 1834·0, we get for 1877—

$$N = 47^\circ\ 55' \qquad I = 39^\circ\ 46'.$$

A SOUTHERN COMET (?).—The *Wanganui Herald* (N.Z.), of January 20, says:—"What appeared to be a small comet was visible in the south, in the constellation of Argo Navis, for about two hours last night, the rising moon rendering it invisible. It was very small, and appeared to be rapidly moving towards the east." The summary number of the *Melbourne Argus* of January 25 has no reference to the visibility of a comet, and the above notice at present requires confirmation.

PROF. FLOWER'S HUNTERIAN LECTURES ON THE RELATION OF EXTINCT TO EXISTING MAMMALIA[1]

VI.

THE order *Cetacea* is one of peculiar interest, having many specialities of structure, and being sharply defined from all other groups, with no outlying or doubtful forms at present known. Being purely aquatic animals, and all of considerable size, their remains are more readily preserved than those of some other orders. None, however, have been met with in the well-explored deposits of the cretaceous seas, or indeed in any European strata (with the doubtful exception of the cervical vertebræ of *Palæocetus* from Ely), earlier than the Miocene. Abundant remains are, however, found in various Miocene and Pliocene marine beds, notably at Antwerp, in many parts of France, Germany, especially the Vienna basin, Italy, and South Russia. They are also found, though in a less perfect condition, in the crags of the east of England. In the Eocene deposits of the eastern states of America, the strange and gigantic Zeuglodons occupy the place of the ordinary Cetacea, which occur in the Miocene and later ages.

Among the existing members of the order there are two very distinct types, the toothed whales or *Odontocetes*,

[1] Abstract of a course of lectures delivered at the Royal College of Surgeons "On the Relation of Extinct to Existing Mammalia, with Special Reference to the Derivative Hypothesis," in conclusion of the course of 1873. (See Reports in NATURE for that year.) Continued from p. 410.

and the baleen whales or *Mysticetes*, and none of the known extinct forms present true transitional or intermediate characters; but it must be remembered that even such remains as have been already collected have not yet been thoroughly worked out. The Mysticetes appear, at first sight, the most specialised and aberrant, in the absence of teeth, in the presence of whalebone or baleen, in the form and size of the mouth; but as we see in other groups, dental characters and all such as relate to the prehension of food generally, are essentially adaptive, and consequently plastic or prone to variation, and hence cannot well be relied upon as tests of affinity. In another character, also adaptive, the laxity of the connection of the ribs with the vertebral column and with the sternum, and the reduction of that bone in size, allowing great freedom of expansion of the thoracic cavity, for prolonged immersion beneath the water, the Mysticetes have passed beyond the Odontocetes in specialisation. On the other hand, the great symmetry of the skull, the more anterior position of the nares, and their double external orifice, the form of the nasal bones, the presence of a distinctly developed olfactory organ, the mode of attachment of the periotic bone to the cranium, the presence of a cæcum, and the regular arrangement of the alimentary canal, the more normal characters of the manus and the better development of the muscles attached to it, and the presence (in many species at least) of parts representing a hind limb, all show less deviation from the general mammalian type than is presented by the Odontocetes. Taking all their structural characters into consideration, as well as what we know of their past history, it does not appear that we can consider either type to have been derived from the other, at all events in the form in which we see it now, but must rather view them as parallel groups.

Among the Mysticetes, in the especially distinguishing characters of the division, the *Balænopteræ* are less specialised than the *Balænæ*, which in the greater size of the head, the length and compression of the rostrum, the development of the baleen, and the shortness of the cervical region, are exaggerated types of the former, and yet they retain more fully some primitive characters, as the better development of the hind limb, the pentadactylous manus, and the absence of a dorsal fin. Both forms are found distinct in a fossil state as far back as the early Pliocene, but generally represented by smaller species than those now existing. The Mysticetes of the Miocene seas were, as far as we know at present, only *Balænopteræ*, some of which (*Cetotherium*, Brandt) were, in the elongated flattened form of the nasal bones, the greater distance between the occipital and frontal bones at the top of the head, and the greater length of the cervical vertebræ more generalised than those now existing. In the form of the mandible they are considered by Van Beneden to present more approximation to the Cetodonts.

Among the toothed whales, the earliest known form, the *Zeuglodon*, was far the most aberrant, approaching in the structure of its skull and teeth to a more generalised but very low carnivorous type. In smallness of cerebral cavity, compared to the mouth and other parts of the skull, it is as far below all other known cetaceans, as the singular *Arctocyon primævus* is below all carnivores. One could quite imagine that the skull of a very degraded seal would present many features in common with Zeuglodon, and this is the only near link we seem to possess between the Cetacea and the rest of the animal world. All the resemblances which some naturalists have seen between the skull of Zeuglodon and the Sirenia are purely superficial and imaginary. The forward position of the narial aperture and the length and flatness of the nasal bones which this animal possesses in common with (though to a greater extent than) the Mysticetes, we may suppose to be common primitive cetacean characters, though completely lost in all other known Odontocetes. Even the Squalodons, which in dental characters so much resemble the Zeuglodons as to have been placed in the same genus by some zoologists, agree in their essential cranial characters with the ordinary dolphins. They are, in fact, dolphins with double-rooted molar teeth, peculiar to the Miocene formations of Europe and America. Among existing dolphins, *Platanista* has been considered to conform most to the general type of mammalian structure. It is therefore interesting to find a similar form (*Champsodelphis*) well represented among the earliest fossil remains of Cetaceans in Europe, and others abundant in North America. Apart from these the greater number of toothed whales range themselves under the two principal heads of *Ziphioids* or *Physeteroids*, and *Delphinoids*. The former are an ancient group, of which the remains are exceedingly numerous in the Antwerp and Norwich crags, and of which the existing sperm whale is a highly modified and specialised form. Among the latter, *Delphinus* and its various modifications may be regarded as the dominating type of Cetaceans at the present day, abundant in slightly differentiated species, and abundant in individuals. They are in this respect to the rest of the order, much as the hollow-horned ruminants are to the Ungulates.

It seems in vain at present to speculate upon the origin of the Cetacea. They present no marks of closer affinity to the lower classes of Vertebrates than do the rest of their own class. Indeed, in all that characteristically distinguishes a Mammal from the oviparous Vertebrates, especially in the nervous and reproductive systems, they are far above many other groups of the class. There is no existing order of land mammals to which they can be said to be decidedly and unquestionably allied. Their agreement with the *Sirenia* is mainly in modifications of structure adapted for a somewhat similar mode of existence, while in many essentials the difference between them is as wide as that between any other two orders. Taking into consideration many of their habits, and their food, and bearing *Zeuglodon* in mind, a relationship to the Carnivora through the seals seems indicated; but if the mode of development has the weight many modern zoologists are disposed to assign to it, their affinities would be rather with the Ungulates, an order from which, on other grounds, they are far removed.

(*To be continued.*)

ON REPULSION RESULTING FROM RADIATION.—PART IV.[1]

In this paper the author describes experiments on the repulsion produced by the different rays of the solar spectrum. The apparatus employed is a horizontal beam suspended by a glass fibre, and having square pieces of pith at each end coated with lampblack. The whole is fitted up and hermetically sealed in glass, and connected with an improved mercury-pump. In front of the square of pith at one end a quartz window is cemented to the apparatus; and the movements of the beam, when radiation falls on the pith, are observed by a reflected ray of light on a millimetre-scale. The apparatus was fitted up in a room specially devoted to it, and was protected on all sides, except where the rays of light had to pass, with cotton-wool and large bottles of water. A heliostat reflected in a constant direction a beam of sunlight, which was received on an appropriate arrangement of slit, lenses, and prisms for projecting a pure spectrum. Results were obtained in the months of July, August, and September; and they are given in the paper graphically as a curve, the maximum being in the ultra-red, and the minimum in the ultra-violet. Taking the maximum at

[1] Abstract of a paper read befor the Royal Society, Feb. 10, 1876, by William Crookes, F.R.S., &c.

100, the following are the mechanical values of the different colours of the spectrum :—

Ultra-red	100
Extreme red	85
Red	73
Orange	66
Yellow	57
Green	41
Blue	22
Indigo	$8\frac{1}{4}$
Violet	6
Ultra-violet	5

A comparison of these figures with those usually given in text-books to represent the distribution of heat in the spectrum is a sufficient proof that the mechanical action of radiation is as much a function of the luminous rays as it is of the dark heat-rays.

The author discusses the question, "Is the effect due to heat or to light?" There is no real difference between heat and light; all we can take account of is difference of wave-length; and a ray of a definite refrangibility cannot be split up into two rays, one being heat and one light. Take, for instance, a ray of definite refrangibility in the red. Falling on a thermometer it shows the action of heat, on a thermopile it produces an electric current, to the eye it appears as light and colour, on a photographic plate it causes chemical action, and on the suspended pith it causes motion. But all these actions are inseparable attributes of the ray of that particular wave-length, and are not evidence of separate identities.

The author enters into some theoretical explanations of the action of the different parts of the spectrum, but these cannot well be given in abstract.

An experiment is described by which sunlight was filtered through alum, glass, and water screens, so as to cut off the whole of the ultra-red or dark-heat rays. The ray of light which was thus freed from dark heat was allowed to fall on the pith surface of the torsion-apparatus, when it produced a deflection of 105°. On interposing a solution of iodine in disulphide of carbon the deflection fell to 2°, showing that the previous action was almost entirely due to *light*. With a candle tried under the same circumstances, the light filtered from dark heat produced a deflection of 37°, which was reduced to 5° by interposing the opaque solution of iodine.

In order to obtain comparative results among discs of pith coated with lampblack and with other substances, a torsion-apparatus was constructed in which two or more discs could be exposed one after the other to a standard light. One disc always being lampblacked pith, the other discs could be changed so as to get comparisons of action. If the action of radiation from a candle on the lampblacked disc be taken as 100, the following are the proportions obtained :—

On Lampblacked pith	100
Iodide of palladium	87·3
Precipitated silver	56
Amorphous phosphorus	40
Sulphate of baryta	37
Milk of sulphur	31
Red oxide of iron	28
Scarlet iodide of mercury and copper ...	22
Lampblacked silver	18
White pith	18
Carbonate of lead	13
Rock-salt	6·5
Glass	6·5

In consequence of some experiments tried by Professors Tait and Dewar, and published in NATURE, vol. xii. p. 217, the author fitted up a very sensitive apparatus for the purpose of carefully examining the action of radiation on alum, rock-salt, and glass. The source of radiation was a candle. Perfectly transparent and highly polished plates of the same size were used, and the deflection was made evident by an index ray of light. Taking the action on the alum at 100, that on the rock-salt in five successive experiments was 81, 77·3, 71, 62·5, 60·4. This increasing action on the alum was found to be caused by efflorescence, which took place rapidly in the vacuum, and rendered the crystal partially opaque. A fresh alum plate being taken, this and the rock salt were coated with lampblack and replaced in the apparatus, the black side away from the source of radiation, so that the radiation would pass through the crystal before reaching the lampblack. The action of radiation was in the proportion of blacked alum 100 to blacked rock-salt 73.

Rock-salt and glass were next tested against each other *in vacuo* in a torsion-balance. Professors Dewar and Tait say that rock-salt is inactive when the beam from a candle is thrown on it, while a glass disc is active. The author has failed to corroborate these results; he found the mean of several concordant observations to be—rock-salt 39, glass 40.

The Measurement of the Force.—The author describes a torsion-balance in which he is enabled to weigh the force of radiation from a candle, and give it in decimals of a grain. The principle of the instrument is that of W. Ritchie's torsion-balance, described in the Philosophical Transactions for 1830. The construction is somewhat complicated, and cannot be well described without reference to the diagrams which accompany the original paper. A light beam, having two square inches of pith at one end, is balanced on a very fine fibre of glass stretched horizontally in a tube, one end of the fibre being connected with a torsion-handle passing through the tube, and indicating angular movements on a graduated circle. The beam is cemented to the torsion-fibre, and the whole is enclosed in glass and connected with the mercury-pump and exhausted as perfectly as possible. A weight of 0·01 grain is so arranged that it can be placed on the pith or removed from it at pleasure. A ray of light from a lamp reflected from a mirror in the centre of the beam to a millimetre-scale 4 feet off shows the slightest movement. When the reflected ray points to zero, a turn of the torsion-handle in one or the other direction will raise or depress the pith end of the beam, and thus cause the index ray to travel along the scale to the right or to the left. If a small weight is placed on one end so as to depress it, and the torsion-handle is then turned, the tendency of the glass fibre to untwist itself will ultimately balance the downward pressure of the weight, and will again bring the index ray to zero. It was found that when the weight of the $\frac{1}{100}$ of a grain was placed on the pith surface, the torsion-handle had to be turned twenty-seven revolutions and 353°, or 10073° before the beam became horizontal. The downward pressure of the $\frac{1}{100}$ of a grain was therefore equivalent to the force of torsion of the glass thread when twisted through 10073°.

The author next ascertained what was the smallest amount of weight which the balance would indicate. He found that 1° of torsion gave a very decided movement of the index ray of light, a torsion of 10073° balancing the $\frac{1}{100}$ of a grain, while 10074° overbalanced it. The balance will therefore turn to the $\frac{99}{100000000}$ of a grain.

Divide a grain weight into a million parts, place one of them on the pan of the balance, and the beam will be instantly depressed.

Weighed in this balance the mechanical force of a candle 12 inches off was found to be 0·000444 grain; of a candle 6 inches off 0·001772 grain. At half the distance the weight of radiation should be four times, or 0·001776 grain; the difference between theory and experiment being only four millionths of a grain is a sufficient proof that the indications of this instrument, like those of the apparatus previously described by the author, follow rigidly the law of inverse squares. An examination of the differences between the separate observations and the mean shows that the author's estimate of the sensitiveness

of his balance is not excessive, and that in practice it will safely indicate the millionth of a grain.

One observation of the weight of sunlight is given; it was taken on December 13; but the sun was so obscured by thin clouds and haze that it was only equal to 10·2 candles 6 inches off. Calculating from this datum, it is seen that the pressure of sunshine is 2·3 tons per square mile.

The author promises further observations with this instrument, not only in photometry and in the repulsion caused by radiation, but in other branches of science in which the possession of a balance of such incredible delicacy is likely to furnish valuable results.

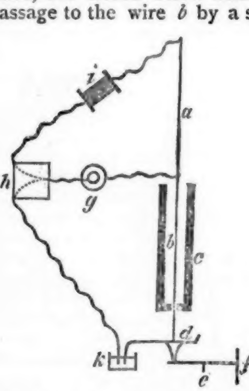
SCIENCE IN GERMANY

(From a German Correspondent.)

A FEW years ago Edlund attempted to decide the question whether the galvanic current is capable of directly altering the volume of a conductor through which it flows, or not, *i.e.*, whether changes of volume were demonstrable that were independent of the heat produced in the conducting wire? The results of his experiments appeared to furnish an affirmative answer to this question. More recently, Streintz published an investigation, the result of which was a confirmation of Edlund's view on the expanding power of the galvanic current. The expanding action found by Edlund was from 2·8 to 6·5 per cent. of the action of the heat simultaneously produced; that found by Streintz was considerably greater. In soft iron it amounted to 27 per cent. of the action of the heat.

From the fundamental importance which attaches to this question, in relation to the theory of galvanism, and from the difficulty of demonstrating the volume-changes referred to, apart from the actions of the heat simultaneously produced, it was desirable that the subject should be investigated by a method as free from error as possible. Such an investigation has lately been carried out by Herr Exner, in Vienna. The essential points of his method are as follows:—

Two pieces, *a* and *b*, of the same wire, about equally long, were suspended vertically one over the other, as indicated schematically in the figure. The lower piece, *b*, passed centrally through a glass tube, *c*, which was quite open above, but closed below with a cork, which merely gave passage to the wire *b* by a short glass tube



(2mm. wide) inserted in it. From the lower end of the wire *b* hung the plate *d* for holding weights. This was furnished at its base with a sharp iron point, meant to act on one arm of a lever which could be turned about *e*, while the other arm bore the mirror *f* at right-angles to its axis, and so in a vertical plane. If the image of a vertical scale were observed in this mirror with a telescope, the least change in length of the wires *a* and *b* could thereby be perceived. From the point of connection between *a* and *b* a wire was connected with the battery *g*. The other pole of the battery was connected with the commutator *h*, and thus the current could be sent either through the rheostat *i* to the suspending point of the wire *a* and through the latter back to the battery, or on the other side to the mercury cup *k*, in which is dipped the bent end of a short copper wire soldered to the plate, *d*, establishing thus a conductive connection between the commutator *h* and the wire *b*. Through the latter the current then went back to the battery. One could thus easily send the current successively through each of the two wire-pieces, *a* and *b*, separately, and so observe the elongation experienced by each. Since, as has been said, the two pieces *a* and *b* were not exactly equal in length, their elongations were also not exactly equal; to make them equal, the rheostat *i* was inserted, by which the resistance in the circuit *g h i a g* was so regulated, that with unchanged battery the successively observed elongations of *a* and *b* were the same. Water was now allowed to pass through the glass tube *c*, in order to take away as much as possible of the heat produced in the wire *b* by the galvanic current. If, now, the current passed through *b*, only the elongation which might occur independently of the heat action of the current would be observed, the heat produced being removed by the flowing water.[1] If, however, the current passed through *a*, both an elongation produced in *a* through direct action of the current, and the elongation through action of heat would be observed at the same time. [These experiments might of course also be made with only one piece of wire, *e.g.* *b*. The second piece *a*, serves only for making the observations more quickly in succession.]

It was found that the galvanic expansion expressed in percentage of the heat-expansion was only about 1·2 to 2·2 per cent.; and no connection was recognisable with the nature of the metal employed. If it be considered that these values, of course, can only be an upper limit, it will follow from the smallness of the effect obtained that there is no sufficient ground for the hypothesis of a special expansion-power of the galvanic current. There can hardly be any doubt that the slight expansion which the water-inclosed wire still shows is simply and alone due to the heat remaining in it. W.

THE INTERNATIONAL METRIC COMMISSION AT PARIS

IN previous numbers of NATURE[2] some information has been given of the proceedings of the International Metric Commission of Paris, and of the progress of their work in providing new international standards of the metric system. The construction of the new standard metres and kilogrammes of platinum-iridium, which was entrusted to the French section of the Commission, is now approaching completion, and their comparisons with the old standards of the Archives and with each other will probably be commenced early this spring.

It has been already explained that the definitive verification of the new standards was entrusted by the Commission to a permanent committee of twelve of their members, each representing one of the principal civilised countries interested. For the purposes of providing the committee with the necessary means of exercising their duties, and of giving an authoritative international character to the new standards, and to the regulations to be adopted for the custody and use of the new international metric prototypes, a diplomatic conference was held at Paris in March 1875, when a convention was entered into for effecting these objects.

Papers relating to the meeting and proceedings of this diplomatic conference, drawn up by Mr. Chisholm, the Warden of the Standards, who was the representative of

[1] It may happen that the heat of the wire is not entirely carried off by the flowing water.

[2] Vol. vii. pp. 197 and 237; vol. viii. p. 403; vol. x. p. 130.

Great Britain at the Conference, were laid before both Houses of Parliament last session by Her Majesty's command. Some further information upon the subject is contained in the Ninth Annual Report of the Warden of the Standards, recently published.

The following is a summary of the terms of the Convention, which bears date Paris, May 20, 1875.

By the two first Articles the high contracting parties agree to found and maintain at their common expense an International Bureau of Weights and Measures, scientific and permanent, its seat to be at Paris. The French Government will undertake to facilitate the acquisition, or if requisite, the construction of a building to be specially appropriated for this purpose.

Article 3 fixes the conditions under which the International Bureau will execute its functions. It is to be placed under the exclusive direction and superintendence of an International Committee of Weights and Measures, which itself is to be under the authority of a General Conference of Weights and Measures formed of delegates from all the contracting Governments.

The President for the time being of the Academy of Sciences at Paris is to be the President of the General Conference of Weights and Measures. But this body will not be called into existence until the verification of all the new standards shall have been completed, when it will be convoked for the purposes of sanctioning them and their distribution. All the Governments who send delegates to the International Metric Commission will be entitled to be represented at the General Conference.

Articles 5 and 6 relate to the organisation of the International Bureau, the International Committee, and the Conference General. The duties of the International Bureau are specified as follows:—The verification and conservation of the new metric prototypes of the metre and kilogramme, the construction of which has been entrusted to the French Section of the International Metric Commission; the verification of all the copies of these prototypes and their periodical comparisons; the verification and comparison of geodesical measuring instruments; and the comparison and verification of standards and scales of precision.

Articles 7, 9, and 10 fix the establishment of the International Bureau and the mode of defraying the expenses by contributions from all the contracting governments, according to a scale based on the respective amounts of their actual population.

These expenses are limited by Articles 5 and 6 of the Regulations annexed to the Convention to the total amount of 400,000 francs (16,000*l.*) for establishing the Bureau and providing it with all the requisite instruments, together with an annual sum not exceeding 100,000 francs (4,000*l.*) for the current expenditure, reducible to 50,000 francs (2,000*l.*), after the completion and distribution of the new national metric standards.

Articles 11, 12, 13 of the Convention reserve the right to every other civilised state to take part in it, under specified conditions; and enable the contracting parties by common agreement to make all such modifications in the terms of the Convention as may be found by experience useful; they also allow any of the contracting parties to withdraw from the Convention at the expiration of a term of twelve years.

A series of Regulations are annexed to the Convention, which fix the details of the organisation of the International Bureau, and of the composition and functions of the International Metric Committee and of the General Conference. Some transitory provisions are also annexed, relating to the completion of the construction of the new standards by the French Section, and their verification and distribution; as well as the mode of constituting the new International Committee and the General Conference.

At the Conference, the sittings of which were continued during March and April 1875, twenty of the principal civilised countries were represented, viz.:—Germany, Argentine Republic, Austria and Hungary, Belgium, Brazil, Denmark, Spain, United States of America, France, Great Britain, Greece, Italy, Holland, Peru, Portugal, Russia, Sweden and Norway, Switzerland, Turkey, and Venezuela.

The Convention was signed by the plenipotentiaries of seventeen of these countries. The Governments of three countries—Great Britain, Holland, and Greece—declined to take part in the Convention. They were willing to take part in it, and to contribute towards the expenses of the International Bureau, if its objects were limited to the verification of the new metric standards and the custody and use of the new prototypes, but they refused to take part in a Convention which established a permanent international institution for other and larger scientific objects, and for promoting the progress of the Metric System.

Further grounds for the refusal of the British Government were that by the terms of the Convention the International Metric Commission, to which delegates from this country were appointed by the Government, was virtually suppressed, and its functions and duties transferred to the new Committee; that the *propagation* of the metric system was declared to be one of the primary duties of the new General Conference, thus sanctioning the objectionable precedent of directly authorising a scientific body to interfere with national usages in countries where the metric system has not been adopted.

The result of their Governments declining to take part in the Convention has been that the representatives of Great Britain and of Holland have been compelled to decline to act upon the new International Committee, which was fixed by the Convention to be formed of the twelve members of the Permanent Committee, with the addition of the representatives of Italy and Switzerland. The Committee now consists of the following twelve members, who are to have the direction of the International Bureau:—

General Ibañez,	representing	Spain, President.
Dr. Förster,	"	Germany.
Dr. J. Herr,	"	Austria.
M. Stas,	"	Belgium.
General Morin,	"	France.
Prof. Hilgard,	"	United States of America.
Dr. Wild,	"	Russia.
General Baron Wrede,	"	Sweden.
Prof. Broch,	"	Norway.
Husny Bey,	"	Turkey.
Prof. Govi,	"	Italy.
Dr. Hirsch,	"	Switzerland, Secretary.

Under the terms of the Convention, the ratifications were to be exchanged on Dec. 20, 1875, and the committee to enter on their full functions on Jan. 1, 1876. Meanwhile, as soon as the provisional instrument of the Convention was signed by the plenipotentiaries on April 15 last, the Committee were authorised at once to constitute themselves and to make their preliminary arrangements. They accordingly held five meetings and elected their president and secretary, as already stated, and they provisionally appointed Prof. Govi director of the International Bureau. By Article 6 of the Regulations annexed to the Convention, the yearly salary of the secretary to the Committee is fixed at 6,000 francs (240*l.*), and that of the director of the Bureau, who is to reside there, is fixed at 15,000 francs (600*l.*). The Committee at the same time selected the Pavillon Breteuil in the park of St. Cloud as the site of the Bureau.

On Dec. 20, 1875, the representatives of eleven out of the seventeen contracting Governments met at Versailles, and exchanged ratifications of the Convention. It was announced that the Governments of Austria and Hungary, the United States of America, and Portugal, had

not yet been able to obtain the sanction of their Legislatures for the ratification of the Convention, and they requested further time for this purpose. The Argentine Republic and Venezuela also requested further time, and the requests were granted. Brazil alone declined to ratify the Convention and take part in its objects.

Before the adjournment of the late French National Assembly, they passed a law which was introduced by the French Government to grant the Pavillon Breteuil at St. Cloud, with some adjoining land, to the directors of the International Bureau for the purposes of this scientific institution, so long as it shall continue in existence.

The following appear to be the approximate proportions which the several contracting States will have to contribute towards the expenses of the new International Metric Bureau, based on Article 20 of the Regulations, by which the unit of contribution is to be determined from the population of each State, expressed in millions, and multiplied by the coefficient 3 for those countries where the metric system is adopted compulsorily; by 2 where it is adopted permissively; and by 1 for other countries.

States.	Population in Millions.	Coefficient.	Product of Units.	Approximate Contributions.		
				Establishment.	Annual, First Period.	Annual, Second Period.
1. Germany	41	3	123	£2,353	£587	£294
2. Argentine Republic	2	1	2	38	10	5
3. { Austria	20	3	60 }	2,066	516	258
Hungary	16	3	48 }			
4. Belgium	5	3	15	287	72	36
5. Denmark	2	1	2	38	10	5
6. Spain	25	3	75	1,434	358	179
7. United States of America	42	2	84	1,607	402	201
8. France	38	3	114	2,181	545	272
9. Italy	27	3	81	1,550	387	194
10. Peru	3	3	9	172	43	21
11. Portugal	7	3	21	402	100	50
12. Russia...	83	1	83	1,598	400	200
13. { Sweden	4	1	4	76	20	10
Norway	2	1	2	38	10	5
14. Switzerland ...	3	3	9	172	43	21
15. Turkey	38	3	102	1,950	487	244
16. Venezuela	2	1	2	38	10	5
	360	—	836	£16,000	£4,000	£2,000
Approximate units of contribution ...				£19	£5	£2 10

If Great Britain had taken part in the Convention, the contribution from this country towards the expenses of establishing the International Bureau would have been about 600*l*., and towards the annual expenditure for the first period about 150*l*. a year.

The International Committee are now therefore in a position at once to commence operations. They are to meet at least once a year, and between their session can deliberate and pass resolutions by correspondence. They considered that at least a year must elapse from Jan. 1, 1876, before their new building and instruments will be ready for use. They therefore passed a resolution charging their executive to notify to the French Section that the Committee would not be prepared to commence their comparisons and verifications of the new Metric Standards before the spring of 1877. This will give plenty of time to the French Section to complete the construction of these new Standards and to make all such comparisons with the Standards of the Archives and with each other as may be necessary to ascertain their values with the requisite precision, before delivering the Standards to the International Committee for definitive verification. H. W. CHISHOLM

PHYSICAL SCIENCE IN SCHOOLS

IT may contribute profitably to the discussion of the subject of Physical Science in Schools, if I state briefly the experience of an effort, extending over twenty-eight years, to give this subject a prominent, if not its merited place in our work. I may say that my boys rarely go to a university, and are almost wholly absorbed in professions, manufactures, and commerce.

Mr. Tuckwell's propositions (NATURE, vol. xiii., p. 412) are good, but perhaps the following modifications are better:—

1. The business of a school is general education; the business of a university is special education.

2. The branches of study for the general education of a school should be Language, Mathematics, Natural Science, and Art.

3. Some knowledge of each of these should be imposed on every pupil; but each pupil should be allowed to apply himself chiefly to that branch of study for which he shows the most natural aptitude, and which therefore will *to him* be the best *means* of education.

4. The matriculation examinations for entrance to the universities should require a fixed standard of knowledge of all these branches of education; and give equal honours for excellence in either.

I think this would place science on an equal footing with language and mathematics, in both school and university, and would in due time relieve us of the conventional pedantry which regards language as the only sufficient standard of an educated man, and ignorance of the simplest elements of science as no disgrace.

Now for our own practice and experience. At nine or ten years of age our boys get simple lessons on wild flowers, which they collect, are taught to examine and describe, and write a simple account of; necessary help of course being given.

On graduating to the upper school, which they do from ten to twelve years of age, they get three hours a week for descriptive lessons on botanical and zoological subjects, with reproductions and as much of classification as is practicable; and on meteorological phenomena and heat, illustrated by the daily and seasonal variations that affect themselves. The object here is to cultivate the observing powers, to induce discrimination of distinctive features, and to promote a thoughtful apprehension of the most easily discerned natural phenomena.

In the next grade four hours are given to science, and the study becomes more special. Mechanics is the subject taken, and if this subject be treated simply, and care be taken not to overrun the mathematical knowledge, it may be made sufficiently attractive, and a valuable means of thoughtful training.

In the next grade two hours a week are taken from mechanics, and chemistry is begun.

In the next grade the other two hours are taken from mechanics, and given to physiology—1. Vegetable; 2. Animal; so that the subjects here are physiology and chemistry with manipulation. In this grade, also, one hour a week is taken from the two commonly given to geography, and given to political economy. The boys in this grade will also often give special time to chemical manipulation, and to practical work in physiology. We have also workshops where a considerable amount of very good work in both wood and metal is regularly turned out.

It may be that I am rather exacting on my own efforts, but I have never been satisfied with our science teaching, and the current discussion of the subject in NATURE has added very materially to my dissatisfaction. To be dis-

satisfied with one's work, however, is one thing; how to make it better is quite another. In chemistry we have perhaps all the means commonly employed. In physiology we can get objective illustrations brought into the class-room, and can use the microscope and diagrams. In mechanics our workshops supply practical work, but for the class-room we have only the most homely practical examples, and I know of no apparatus that is not cumbrous, needlessly costly, or ineffectual. Johnstone's admirable illustrations and the black-board are our chief agents. I would travel far to get a practical knowledge of means and methods by which we could improve our own.

I believe that such knowledge as I have indicated may be profitably given even to very young boys. They learn thereby to distinguish the precise features and qualities of natural objects, and the conditions of ordinary phenomena; and such teaching undoubtedly exercises in the best way the observing powers which develop much earlier than the reflective faculty. I am inclined to say that teaching elementary science to boys from ten to thirteen is a greater success than teaching grammar, *i.e.*, that the principles involved are more easily seen, excite more interest, and become therefore a better mental discipline. We rarely have boys come to us with any knowledge of science, and when they have, it has generally been acquired from lectures, and is worthless as a means of education. We do not lecture, but do real hard class-work, and take periodical examinations on this work, giving it equal value in these and our grade examinations with language and mathematics. We have no reason to believe that this work interferes with or deteriorates the work in language and mathematics, in which subjects we find our boys quite equal, and, except in very rare cases, I may say, superior to incomers of like power, and who have had no science teaching.

The great number of men eminent for their vast scientific attainments, who have achieved this eminence in spite of our non-scientific, I may almost say anti-scientific, system of education, clearly indicates that many of us have an inherent scientific power or genius surpassing our power in any other direction. I plead for such that they have the same chance of being floated on their scientific voyage as the linguist and the mathematician have on theirs: and I have seen no satisfactory plea why they should not. Value for value I claim for the science man a higher status in our present social life than is due to either linguist or mathematician.

My experience as a schoolmaster has revealed to me many cases where the talent for language or mathematics has been so low that the education effected by these has been of the meanest kind; or where the incessant failure has produced a stolid ignorance, a kind of mental paralysis, most disheartening to all concerned. Such cases have come into my hands, and I have seen intelligence rekindled, and mental power aroused by simple science teaching, and the power even for other subjects enhanced thereby. I plead for these feeble ones. Is it not a crime to them to keep the mind fixed on what to them is abstruse and unintelligible, and to shut against them the inspiring book of nature, which may contain the only intellectual sunshine of which their being is susceptible?

Allesley Park College, Coventry T. WYLES

I shall be obliged if you will permit me to remind Mr. Wilson of certain passages in his article contained in Essays on a Liberal Education."

In his letter (NATURE, vol. xiii. p. 373) Mr. Wilson writes:—

"I maintain, after trial, that it is unwise, and unscientific from an educational point of view, to attempt to teach science at schools to boys till they have attained a certain standard of knowledge in arithmetic, and a certain power of reasoning and language as shown by their attainments in geometry and Latin. Let science be held before them as a thing to be enjoyed when they are older and more advanced. It is spoiled for them, and they are spoiled for it by its being taught them too soon. The dicta of men like Faraday and Sir John Lubbock and Roscoe are misleading opinion on this point, and I wish to record my protest against them."

But in "Essays," &c., Mr. Wilson wrote:—"Moreover, the kind of knowledge that science offers is not only wide and interesting and elevating, but it is also exact; and this exactness is a very great merit. It is a knowledge of things and not of words. In the education of the upper classes there is too little of positive and exact knowledge. . . . And natural science supplies this want of clearness and certitude better than arithmetic or geometry." And again:—

"But here is even a stronger ground for advocating the introduction of science as an element in all liberal education, and that is its peculiar merit as a means of educating the mind. . . . All that can be said on this point has been said over and over again, and I can contribute nothing except my daily experience that what is said is true. . . . Science is the best teacher of accurate, acute, and exhaustive observation of what is. . . . And of all processes of reasoning it stands alone as the exhaustive illustration."

Giggleswick, April 4 W. MARSHALL WATTS

NOTES

THE date now finally fixed for the opening of the Scientific Loan Exhibition is the 1st of May. This delay is entirely owing to the unexpected richness and variety of the collection. Germany alone sends upwards of 2,500 objects, many of them of the greatest value. Although France has sent some very fine objects for exhibition, she will, on the whole, be rather poorly represented. The Italians are sending all the riches of their storehouse at Florence, including Galileo's telescopes.

ON the 31st ult. a meeting, at which several well-known English biologists were present, took place at the house of Dr. Burdon-Sanderson, at which the advisability of establishing a society or association for the purpose of promoting the progress of physiological research in England, was considered and discussed. Eventually the matter was referred to a committee, who will report to a future meeting; after which some conversation followed as to the question of legislation, the general feeling of those present being that no opposition ought to be made on the part of scientific men to any measure framed in accordance with the recommendations of the Royal Commission.

A MOVEMENT has been organised for erecting a monument to the late Jean Baptiste Donati. The idea originated with the professors of the Physical and Natural History Museum of Florence, and it is proposed to erect the monument in the new observatory of Arcetri, which was in a manner Donati's work. We are sure there are many admirers of Donati in this country who will gladly subscribe to such a monument. The foreign Legations and Consuls of Italy are authorised to receive and transmit subscriptions to the Committee for Donati's monument, to whom they may be sent direct, at the Natural History Museum, Florence.

IT is proposed to raise by subscription a fund for the purpose of establishing a Memorial in honour of the late Daniel Hanbury; the amount of each contribution not to exceed one guinea. The form suggested for the memorial is that of a medal to be called the "Hanbury" medal, to be awarded for original research in the Chemistry and Natural History of Drugs by investigators in any part of the world. Dr. Hooker, Sir George Burrows, Sir James Paget, Sir Robert Christison, Dr. Allman, Dr. Warren de la Rue, Prof. Abel, and Mr. T. Hyde

Hills, have already expressed their cordial approval of the movement, and consented to be placed on the Committee now in course of formation; Prof. Attfield and Messrs. Carteighe and Bremridge are acting provisionally as secretaries.

LIEUT. CAMERON arrived at Liverpool in the *Congo* on Sunday, and as might be expected, was received with the greatest enthusiasm. Besides his relatives there were present the Liverpool municipal authorities, and Mr. Tinne, as representative of the Royal Geographical Society. Lieut. Cameron was attacked by scurvy on reaching the west coast of Africa, but is now to all appearance quite recovered, and looks strong and well. On Monday he was quite the lion of Liverpool, where he was fêted and toasted and justly treated as a hero who has accomplished a great and useful work. He gives a glowing account of the interior of Africa, and hopes that his journeyings will lead to its commercial development. There is no doubt that at the meeting of the Geographical Society next Tuesday, in St. James's Hall, he will meet with a warm reception. On Tuesday he met with a hearty welcome on his return to Shoreham, near Sevenoaks, of which place his father is vicar.

A COMMISSION of the Geographical Society of Paris has awarded the Society's Great Medal to Dr. Nachtigal, the German North African explorer. It is stated that owing to the arrival of Lieut. Cameron, steps will be taken by the Society to award extraordinary honours to the British explorer who has done so much for African discovery. The anniversary meeting of the Geographical Society is to take place on the 9th of April.

A DISCOVERY of great importance to prehistoric archæology has just been made in France. On March 2, while some workmen were excavating in a quarry of Jurassic limestone, near the little commune of Cravanche, about three kilometres N.W. of Belfort, at the base of a hillock, they came upon a small opening which was found to lead into a cave of large dimensions. On entering the cavern its floor was found to be covered with human bones, so disposed as to lead to the belief that the cave formed a prehistoric place of sepulture. Magnificent stalagmitic columns rise from the floor, but without corresponding stalactites. It would seem as if these columns were partly natural and partly artificial, as they appear to be disposed in a sort of systematic method, after the form of dolmens. It is in these cavities that the multitude of bones have been found. The skulls are raised slightly above the level of the rest of the bodies, which appear to be in a somewhat bent position. Several polished flint weapons have also been found, three beautiful ornamented vases in the form of urns, polished stone bracelets, and a mat of plaited rushes. The cave itself is calculated to be about 36 metres long and about 12 in breadth and height; and numerous galleries, or side-caverns, run off the main one. Immediately on the discovery of the cavern, the authorities of Belfort took measures to guard it and the treasures it contains in the interests of science, and M. Félix Voulot, who has given great attention to the subject of prehistoric archæology, has been charged with carrying on the researches. M. Voulot hopes to be able to disengage from the stalagmitic covering at least one entire skeleton. There is no doubt that we have here important remains of the polished stone period, but it is confidently expected that further research will bring to light relics of a much older period; indeed the writer in the *Revue Scientifique*, of April 1, from which the above details are taken, hopes that remains will be found not only belonging to the Tertiary, but even to the Cretaceous period. The cavern is situated in a bed of one of the lower strata of the Jurassic period (lower oolite), "on the *exact limit* of the shore of the ancient Jurassic sea."

THE following arrangements of the Royal Institution lectures after Easter have been announced:—Prof. P. M. Duncan, F.R.S.: Four lectures on the Comparative Geology and former Physical Geographies of India, Australia, and South Africa; on Tuesdays, April 25 to May 16. Prof. Tyndall, D.C.L., LL.D., F.R.S.: Seven lectures on Voltaic Electricity; on Thursdays, April 27 to June 8. Prof. W. K. Clifford, F.R.S.: Two lectures on the Present Relations of Science and Philosophy; on Saturdays, April 29 and May 6. Prof. W. G. Adams, F.R.S.: Three lectures on some of Wheatstone's Discoveries and Inventions; on Tuesdays, May 23 to June 6. Frederick J. Furnivall: Two lectures on Chaucer; on Saturdays, May 13 and 20. J. A. Symonds: Three lectures on the Medici in relation to the Renaissance; on Saturdays, May 27 to June 10. The Friday evening meetings will be resumed on April 28, at 8 P.M., when Prof. Gladstone will give a discourse on Methods of Chemical Decomposition illustrated by Water. Discourses will probably be given by G. J. Romanes, W. Froude, C. T. Newton, J. F. Moulton, Sir John Lubbock, and Prof. Tyndall.

IN the Mauritius *Overland Commercial Gazette* of March 3, Mr. Meldrum refers to a small but violent cyclone which swept over the island on Feb. 19. Although its approach was announced by the Observatory on the previous day, it was only on the evening of the 18th that it was known with certainty that a gale was imminent. The wind was at its strongest between 2 and 7 A.M. on the 19th, and during that interval it had a velocity of 66·5 to 77·5 miles per hour. The lowest barometric pressure was 29·102 at 4 A.M. on the 19th, and during the night the mercury oscillated considerably. Between 3 and 4.30 A.M. several flashes of lightning were observed. The rainfall at the Observatory was only 2·94 inches. The chief interest of this cyclone is an unusual rate of progression, and the suddenness with which it came on. Its centre passed N. and N.W. of the island at a distance of forty to fifty miles. On the 23rd and 24th it was evident that another cyclone was approaching; fortunately, however, after approaching from the N.E. to about 160 miles of the colony, the storm recurved to the southward, otherwise the colony would have suffered severely. Mr. Meldrum mentions, as an interesting coincidence, that from the 10th to about the 22nd a large sun-spot was visible.

AT the concluding Gilchrist Lecture to the Students of the St. Thomas Charterhouse School of Science, Mr. W. E. Forster, M.P., occupied the chair, and spoke of the importance of teaching science in elementary schools, and of the ungrounded apprehension that thereby rudimentary education would be neglected. He believed it was the duty of the country to give children in elementary schools as much learning in science as they could obtain while they remained there, and should not be afraid to teach them science because their station in life was humble. He believed that if science were substantially and practically taught it would do nothing but good. He was well aware that some of those who had taken part in the education movement thought that if science were taught in elementary schools there would be increased danger of the neglect of good elementary teaching; but he did not think there was any ground for such apprehensions; on the contrary, he thought it would be found that, generally speaking, where science was taught best there was the best teaching of such elements as reading, writing, and arithmetic.

WE regret to hear of the death, at the age of sixty years, of Dr. Letheby, the well-known analyst, for many years Medical Officer of Health for the City of London, and Lecturer on Chemistry at the London Hospital. Dr. Letheby was a Fellow of the Linnean and Chemical Societies.

THE death is announced of Signor Severino Grattoni, the Italian engineer, who, amid great obstacles, carried out the execution of the Mont Cenis Tunnel.

THE Iron and Steel Institute concluded its London Meeting last Friday; the papers read were all of a purely technical

nature. There are now about 900 members of the Institute, the next meeting of which will be held in Leeds in September.

THE Institution of Naval Architects commences this year's session to-day, and will continue its meeting to-morrow and Saturday in the hall of the Society of Arts. Lord Hampton, G.C.B., D.C.L., president, will occupy the chair. Among the many papers to be read are the following:—On the unequal onward motion in the upper and lower currents in the wake of a ship, and the effects of this unequal motion on the action of the screw propeller, by Prof. Osborne Reynolds.—On the theory of pitching, by W. Froude, F.R.S., Vice-president.—On the telegraph-ship *Faraday*, by C. W. Merrifield, F.R.S., Associate Member of Council.—On the propulsion of bodies, by R. Griffiths.—On a new form of hydraulic propeller, by M. E. François.

AT the conclusion of Prof. Huxley's Course of Lectures at Jermyn Street, which we are reporting, Mr. F. Wilson proposed a vote of thanks to the lecturer, and alluded to the state of Prof. Huxley's health. The vote was enthusiastically responded to, and Prof. Huxley, in reply, remarked that the gentleman who had kindly proposed the vote of thanks was under a mistake about his health, he was never better in his life; but, as it had been truly said, "If you had been ill it took at least four years to persuade your friends that you were well again."

THE *Pandora*, Capt. Allen Young's yacht, is now being prepared at Southampton for another voyage to the Arctic regions in search of despatches from the expedition under the command of Capt. Nares. The yacht will be ready for sea in the course of a few weeks.

WE are requested to state that Mr. E. B. Tylor gives his lecture on "Ordeals and Oaths" to morrow evening at the Royal Institution; there seems to have been some misunderstanding as to the date.

WE believe there are a good many people who would wish to visit the *Challenger* on her return to England, while all her equipments are *in situ*, before she is gutted. We are sure that if the authorities are made aware of the existence of such a desire on the part of the public, they will make arrangements to gratify it.

ON Easter Monday and following days the Geologists' Association will make an excursion to Nottingham and Belvoir Castle.

IT is stated as probable that the site of the proposed International Exhibition at Paris in 1878 will be the Bois de Boulogne and Passy.

M. BALARD, the well-known natural philosopher, who has taken part in most of the International Exhibitions that have been held, has died at Paris at the age of seventy-four. M. Balard was a member of the Academy of Sciences for more than thirty years. He was Professor of Chemistry at the Sorbonne. He leaves no written book, but his teaching was much admired.

ON March 25 an earthquake was felt at Algiers and vicinity at 6h. 34m. in the morning. The duration was three seconds. A second movement, less intense, was felt at 7h. 2m. in the afternoon. In the evening a strong storm set in from the S.W., and a deluge of water followed.

WE have received a copy of the rules recently adopted for the Cumberland Association for the Advancement of Literature and Science. The Association consists mainly of a number of local societies in Cumberland, and will hold an annual meeting, at which reports will be read from the affiliated societies, and the objects of the Association furthered by lectures, papers, addresses, discussions, &c. The association intends to publish at its own expense such portions of its own or of any of the associated societies' communications as may be deemed advisable. The objects of the association are laudable, and we wish it success. Its first annual meeting will be held at Whitehaven in May.

WE would recommend to the notice of teachers, the Rev. George Henslow's paper on "The Practical Teaching of Natural Science in Schools," which is published in *The Educational Times* of March 1, and a paper in the April number by the Rev. P. Magniss, on "The Teaching of Natural Philosophy in Schools."

AT the College examination at Downing College, Cambridge, in June, a Foundation Scholarship, of the value of 80*l.* per annum, will be awarded for proficiency in Natural Science. Particulars may be obtained from the tutors of the College.

SEVERAL important German publications have been forwarded to us; we regret that we are unable to do little more than give their titles. They may all be obtained through Messrs. Williams and Norgate, of London and Edinburgh. Dr. Hermann Hager has published a fifth edition of his useful little work, "Das Mikroskop und seine Anwendung" (Berlin: Sprengen). It is intended as a guide to medical men, pharmacists, students, &c., and is largely illustrated.—"Werden und Vergehen" (Berlin: Bornträger) is the title of a work by Carus Sterne, of Berlin, in which an attempt is made to trace the development of the universe from the nebulous "world-cloud" to man. Its tone may be inferred from the fact that it is admiringly dedicated to Prof. Haeckel.—Prof. J. C. G. Lucae has completed his work on the Anatomy of the Seal and Otter, "Die Robbe und die Otter" (Frankfort: Winter), *Phoca vitulina* and *Lutra vulgaris*, a monument of minute research. The first part of this work we noticed in vol. viii., p. 222; the complete work contains thirty-two fine plates.—Dr. M. J. Schleiden has published a curious work on Salt—"Das Salz" (Leipzig: Engelmann), its purpose being to trace the history, the symbolism, and describe the various uses of salt to man. Its main object is to show what an important effect salt has had on human culture.—"Grundriss der anorganischen Chemie" (Leipzig: Voss) is the title of a handbook by Dr. Rudolf Arendt, intended for use in German Middle, Higher, and Training Schools.—Under the title of "Physiologisches Methodik" (Braunschweig: Vieweg) Dr. Richard Gscheidlen, of Breslau University, has published what seems an admirable handbook of practical physiology. It is profusely illustrated with beautifully-executed woodcuts.—Under the title of "Theoretische Kinematik, Grundzüge einer Theorie des Maschinenwesens" (Braunschweig: Vieweg und Sohn), Prof. Reuleaux has published a work on Mechanics which will, no doubt, take a first place among treatises on this subject. Messrs. Macmillan and Co. will shortly publish a translation of Prof. Reuleaux's work.

MESSRS. Williams and Norgate have sent us "Nouveaux Éléments de Physiologie Humaine" (Paris: Baillière), by Prof. H. Beaunis, a work which, we believe, will take a high rank as a text-book.—A French translation of the Guide to Analysis of Water, by Dr. Reichardt, of Jena, has been made by Prof. G. E. Strohl, of Nancy; it is published by Reinwald, of Paris, and sold here by Williams and Norgate.

UNDER the editorship of Mr. R. Brough Smyth there has been published a Descriptive Catalogue of Rocks, Minerals, and Fossils, illustrative of the Geology, Mineralogy, and Mining Resources of Victoria, Australia, intended for exhibition at the Philadelphia International Exhibition. The list contains altogether 880 specimens.

MESSRS. PARKER and Co. have published Prof. Prestwich's lecture "On the Geological Conditions affecting Water Supply to houses and towns, with special reference to the modes of supplying Oxford."

THE Prince of Wales is bringing home with him a large collection of living animals, including, among the most important, two Musk Deer, three Thars, a Manis, three adult Ostriches, four Elephants, five Tigers, three Leopards, sixteen Impeyan Pheasants, more than twenty Tragopans and Cheer Pheasants, several other Deer and Antelopes, together with Fruit Pigeons, Peafowl, &c. These His Royal Highness intends to have exhibited as one collection, and as such they will be deposited in the Gardens of the Zoological Society, a suitable house being in course of erection, and now nearly completed, for their reception.

THE additions to the Zoological Society's Gardens during the past week include a Lesser White-nosed Monkey (*Cercopithecus petaurista*) from West Africa, presented by Mr. F. Ward; a Common Marmoset (*Hapale jacchus*) from South-east Brazil, presented by Mrs. Cleaver; a Wild Sheep (*Ovis burrhel*), an Impeyan Pheasant (*Lophophorus impeyanus*) from the Himalayas, deposited; two Wheatears (*Saxicola œnanthe*), European, purchased; two Cuming's Octodons (*Octodon cumingi*) born in the Gardens.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 23.—"Description of a Mammalian Ovum in an early condition of Development," by E. A. Schäfer, Assist. Prof. of Physiology in Univ. College.

The author describes the ovum of the cat from five, in the stage where they were rendered evident as scarcely perceptible swellings in the cornua uteri. In their long axes they were $\frac{1}{7}$ inch long; in their short, $\frac{1}{15}$ inch. No mesoblast was anywhere present between the hypo- and epiblastic layers, which latter were clearly separable from one another and nearly in contact only in a small part, where only the cells of each were in more than single layers. At this spot also, the cellular elements of each layer being perfectly recognisable, an exquisitely fine pellicle, which in section appears as a mere line, passes over and forms a definite boundary to the outer surface of the hypoblast at the thickened area. This is named the *membrana limitans hypoblastica*, and is found to be perfectly homogeneous and continuous, becoming stained slightly with carmine, and is probably a cuticular formation produced by the underlying cells. This structure seems to have been as yet unnoticed; the mesoblast cells without doubt first appear outside it, which is in favour of the epiblastic origin of that layer.

"On the nature of the force producing the motion of a body exposed to rays of heat and light," by Arthur Schuster, Ph.D., Demonstrator in the Physical Laboratory of Owens College. Communicated by B. Stewart, F.R.S., Professor of Natural Philosophy in Owens College, Manchester.

Mr. Crookes has lately drawn attention to the mechanical action of a source of light on delicately suspended bodies *in vacuo*; I have made a few experiments with the view of finding out the seat of the reaction which evidently must tend to move either the enclosure or the source of light. I have found that the action and reaction is entirely between the light bodies suspended *in vacuo* and the exhausted vessel.

Mr. Crookes' "Light-Mill" was suspended by means of two cocoon fibres, forming a bifilar suspension from the top of a vessel which could be exhausted. A slight movement of the enclosure could be easily detected by means of a concave mirror attached to it. A beam of the oxyhydrogen lamp was concentrated on the light-mill, which then revolved about 200 times a minute.

The light was cut off at the beginning of the experiment by means of a screen, and the position of rest of the glass vessel was read off by means of the dot of light on the scale. The screen was then suddenly removed, and in every case a large deflection of the glass vessel was observed. The vessel was deflected in the opposite direction to that in which the mill turned. When the velocity of the mill had become constant, the vessel returned to its original position of rest, but on suddenly cutting off the light the vessel was again deflected, but in the opposite direction as on starting the experiment. The vessel therefore now turned in the same direction in which the mill turned.

These experiments are easily explained on the assumption that the force acting on the vessel enclosing the light-mill is exactly equal and opposite to that acting on the mill itself. While the velocity of the mill in one direction is increasing, a force acts in the opposite direction on the vessel. When the velocity has become constant, the force which tends to drive the mill round is exactly counterbalanced by the resistance which opposes the motion of the mill. The two forces acting on the vessel will therefore counterbalance, and the vessel will return to its original position of rest. When the light is cut off, the resistance will stop the motion of the mill. The reaction of the resistance will act on the enclosure, and the enclosure will turn in the same direction as the mill.

By means of the reaction on the enclosure I have been able to calculate the strength of the force; and I have found that the pressure on a surface on which the light of equal intensity to that used in my experiments falls is equal to that produced by the weight of a film of water equal in thickness to the length of a wave of violet light.

March 30.—"On the Placentation of the Lemurs," by Wm. Turner, M.B. (Lond.), Professor of Anatomy, University of Edinburgh. Communicated by Prof. Huxley, Sec. R.S.

In the introduction to this memoir a description was given of the observations made by M. Alphonse Milne-Edwards on the gravid uteri of several genera of lemurs. The author then proceeded to describe the gravid uteri of six lemurs which he had received from Dr. Andrew Davidson, of Antananarivo, Madagascar, viz., *Propithecus diadema, Lemur rufipes*, and *Indris brevicaudatus*. He then summarised the conclusions he had arrived at in the course of his dissections, and showed that the placenta in these animals was diffused, and presumably therefore non-deciduate. The paper concluded with a discussion of the bearing of these observations on the classification of the lemurs, and on the theory of the descent of the deciduate mammals from a primæval root-form of Prosiniæ.

"The Residual Charge of the Leyden Jar," by J. Hopkinson, M.A., D.Sc. Communicated by Prof. Sir William Thomson, F.R.S.

Linnean Society, March 16.—Prof. Allman, president, in the chair.—Messrs. Edward R. Alston and David Blair were elected Fellows of the Society.—Dr. J. Anderson communicated a note "On the plastron of the Gangetic Mud-turtle, *Emyda dura*," detailing an instance where eleven instead of the usual nine bones were present. This occurred in an embryo from the egg, and it further appears that ossification and coalescence of the extra pair of bones is coincident with birth.—Mr. A. W. Bennett read a paper "On the rate of growth of the flower-stalk of the hyacinth," in which he showed that the greatest energy of growth is in the lowermost part of the stalk. This agrees with the recorded observations respecting the relative growth of different nodes of a stem, where the greatest energy is always at a considerable distance from the apex. But it offers a contrast to the phenomena exhibited in the submerged flower-stalk of *Vallisneria*, where the greatest energy of growth is in the terminal portion beneath the flower-bud.—Mr. Francis Darwin laid before the Society the results of his observations "On the hygroscopic mechanism by which certain seeds are enabled to bury themselves in the ground." These related chiefly to the Feather-grass, *Stipa pennata*, but similar phenomena obtain in other grasses, in *Anemone montana* and certain of the Geraniaceæ (as Hanstein has recorded, 1868). The essential structures are—a sharp point with reflexed hairs, and a strong woody awn, so bent as to possess a lower vertically helical and an upper horizontal part. With moisture the spiral portion of the awn untwists, causing the horizontal part to revolve, while the flexure between them disappears and thus straightens the awn. A reversal of this process succeeds on the awn becoming dry. In the case of *Stipa* the long, feathery, horizontal part of the awn is easily entangled in low vegetation, and the seed retained vertically, with point on the ground. With wetting the awn untwists, but the horizontal part prevented from revolving, the rotation is transferred to the seed, which latter has superadded pressure of its point, by a conversion of the awn to straighten itself. Again, as the awn dries the seed is not pulled out of the ground, but curiously enough is thrust deeper down; the reflexed hairs being subservient. By such combinations and alternate actions complete burial of the seed ensues. The special advantage of seeds being thus imbedded is obscure; in *Stipa* being unconnected with germination, though possibly a

protection from birds. The effects of changes of temperature Mr. Darwin points out. Hildebrand's and Hanstein's explanations of the torsion he thinks inadequate. Darwin's observations prove it resides in the individual awn-cells. When isolated and dried these latter twist on their own axis, similarly in direction, &c., to the awn and with moisture untwist. Finally, this remarkable power is shown to depend on the molecular structure of the twisting cell-walls. Nägeli and others' researches into twisting cells have not led hitherto to their importance in plant life.—The Secretary briefly referred to a technical paper by the Rev. J. M. Crombie, "On the Lichens of Antarctic America collected by Dr. R. O. Cunningham during the Voyage of H.M.S. *Nassau*, 1867-9." In this ninety-seven species and varieties are recorded: twenty-four of these and a genus, *Endocena*, are attributed to be new.—A discussion on the potato fungus followed. At the President's request Mr. Carruthers reiterated the salient features of Prof. de Bary's recent investigations. He called attention to the difference in thickness of the mycelial threads carrying oospores and antheridia, to the septate character of the threads, and to other points collectively adverse to Mr. W. Smith's views of the "resting spore" of *Peronospora;* De Bary believing two fungi have been confounded. The Rev. M. Berkeley defended Mr. Smith's conclusions as opposed to De Bary's, asserting that as the former, by photographs and drawings from nature, had shown the sexual congress of antheridium and oogonium derived from the unequal sized spawn-threads, no reasonable doubt of their connection existed. Admitting De Bary's extensive knowledge of fungi, Mr. Berkeley, nevertheless, objected to his style of criticism. Mr. Smith himself read a long written reply answering De Bary's objections in detail. He averred that Sadebeck's recent observations supported his own as to two sizes of mycelial threads, and the other objections raised with regard to the oogonia and antheridia. As to septal character of the threads, this belongs to *Peronospora*, those of *Pythium* being destitute in this respect. He further alluded to the warty bodies of Montagne's *Artotrogus*, showing De Bary had misconceived their nature. The Strasbourg professor's animadversion on the life-history of Smith's "resting-spore" being yet incomplete is weakened when the former admits it may take a year to resolve, and as yet only nine months have elapsed since the discovery of the bodies in question. Other remarks pertaining to the "resting-spores" being found in dry leaves, after decay in water, and on perennial mycelium were made, Mr. Smith concluding that De Bary had not entirely comprehended his publications on the subject. Mr. Renny expressed his opinion that the points were not absoutely settled on either side, discrepancies still appearing to him to exist. De Bary's objections were allowable on the ground of his extensive acquaintance with the subject, while possibly Smith may not have given the exact value to what he saw. Mr. T. Dyer suggested that the bodies of the so-called *Artotrogus* may be but mycelial dilatations, and not true oogonia; on this ground a fresh investigation might be necessary to ascertain its relations to the questions at issue. Mr. A. Murray, Mr. Cooke, and Dr. Masters each made a few remarks. Mr. Carruthers, in conclusion, thought Mr. Renny had put the case fairly. De Bary only meant to question Smith's knowledge of the conidia, not that he was ignorant of the potato fungus; he, De Bary, may have misunderstood Smith's drawings, but in the elucidation of facts and truths he certainly could not fairly be accused of hypercriticism, seeing that he himself had carefully watched and studied the development, mode of hosts, &c., since 1874.

Geologists' Association, March 3.—Mr. William Carruthers, F.R.S., president, in the chair.—On the Bagshot sands in the Isle of Sheppey, by Major F. Duncan, D.C.L., F.G.S. A recent section, made with the object of lowering a road, has exposed a considerable part of Bagshot sands, colour pale or light yellow, with clay lines, resting on the top of the dark London clay. The distinctive features are absence of greensands, presence of thin layers and nodules of iron-sandstone, absence of fossils. As there is no discoloration of the sands at the base of the section the author thought they might have been sub-aërial deposits—blown sand; otherwise he considered that there would have been a shading off between the clays and the sands. This theory, the author thought, might serve to explain the well-known variability of this series.—On some rock fragments in the above-described section, by Mr. W. H. Shrubsole. These had been found 6 to 18 inches from the surface and were all igneous, except some specimens of hard sparry limestones. After discussing the possibility of these fragments having been brought to Sheppey by human agency, the author contended that, although their position in the described section may have been due to such a cause, still that they must originally have been conveyed on ice, towards the close of the Glacial epoch, and been stranded whilst Sheppey was emerging from the sea.—Known facts and unknown problems in Arctic geology, by Charles E. De Rance, F.G.S., H.M. Geol. Survey. The existing glacial phenomena of the Arctic regions, Greenland, and Spitzbergen, were described, marine shells of existing species occurring at heights of more than 1,000 feet above the sea, and living marine Crustacea in fresh-water lakes elevated many feet above the sea-level in Polaris Bay. The observations on the discoloration of the waters of the Arctic Ocean were dwelt upon, and the bearing on the phenomena observable on the English Glacial deposits. The crystalline rocks of the north coast of America and the Greenland coasts were referred to the Laurentian system, and the whole of this area stated to have been land, during the Lower Silurian epoch. The Upper Silurian, however, was shown to be well represented in all the islands of the Arctic Archipelago, and the "Ursa Stage" of Prof. Heer, Devonian, or Lower Carboniferous, with coal-seams, to be present in synclinals in the latter, and also to exist in Spitzbergen and Bear Island, as do the overlying mountain limestones. The Lias and Oolitic rocks of the Arctic Islands, East Greenland, and Spitzbergen were described, and the Cretaceous plant-bearing beds of West Greenland, and their associated coal-seams. The Miocene basalts of Mid-Greenland, with their associated plants, were mentioned as probably connected with the basalts of East Greenland, and as ranging to Spitzbergen.

Physical Society, March 25.—Prof. G. C. Foster, F.R.S., president, in the chair.—The following candidates were elected members of the society:—The Marquis of Salisbury, Prof. Liversidge, W. Ackroyd, Tolver Preston, W. Merritt.—Mr. O. J. Lodge, B.Sc., made a communication on the flow of electricity in a plate, in continuation of a paper which he read before the society on Feb. 26. In order to apply the principle of images already described to the flow of electricity in plates bounded by straight lines, it is necessary that the angles of the plate should be aliquot parts of 180°; and, since this condition excludes obtuse angles, the number of rectilinear figures which can be treated is very limited. They are rectangles, equilateral triangles, two cases of right-angled triangles, the two limiting cases of the isosceles triangle for which the equal angles are 0° and 90° respectively, and many cases of the general two-sided polygon or "wedge," including the regular two-sided polygon or "strip." Since the images of a pole in a wedge lie on a circle as in a kaleidoscope, Cotes' property of the circle may be applied to obtain expressions for the potential of any point, and for the electrical resistance of the plate to the flow from any number of point poles situated anywhere in it. The expressions are rather long, but they become simpler in certain special cases which were pointed out. Making the angle of the wedge vanish the expressions modify into corresponding expressions for the strip, the resistance expressions of which always contain hyperbolic trigonometrical functions of the positions of the poles. The potential functions for a circular sector also follow from a general case of the wedge. The general resistance formula, applied to the case of the isosceles right-angled triangle leads to some continued products, all of which are generalisations of Wallis' expressions for $\frac{\pi}{2}$. The product of these products, which is itself of the same form, has been reduced by Mr. J. W. L. Glaisher to the complete elliptic integral usually denoted by K, its modulus being Sin 45°. This quantity appears in all the resistance expressions for right-angled triangles and squares which the author has yet examined. The case of an equilateral triangle leads to more complex and interesting products, which were reduced by Mr. Glaisher to the product of two theta-functions, with Sin 75° as a modulus. When the conditions of flow are known in one rectilinear figure they may be extended to a large number of others by alternate processes of reflecting the plate in one of its own boundaries, and of cutting it along one of its straight flow or equipotential lines. Diagrams of such transition figures were shown. In order to obtain the resistance of a compound conductor by means of the known resistance of its components, it is necessary that the flow conditions in each component shall remain entirely unaffected by their being connected together. Thus if the resistance of a circuit consisting of two wires side by side is to be deduced from the resistance of the wires separately, by the ordinary method of adding their conductivities, it is

necessary either that the wires shall not touch each other, or that if they do, no flow shall pass across the junction. This rule is often overlooked, and the oversight has given rise in certain cases to a notion of electrical "interference." The concluding part of the paper has to do with the flow conditions when fine poles are combined with point poles in a sheet, especially when point electrodes are introduced into a sheet when a uniform current or "river" is flowing across the sheet.—Dr. Guthrie then communicated a fourth paper on salt solutions and attached water. It consists mainly of an account of an examination of the behaviour of a salt solution, when cooled below the freezing point of water. Having shown in previous communications that every salt solution, when of a certain strength solidifies as a whole, at a certain temperature as a cryohydrate, the present research was directed to the determination of the temperatures at which, (1) ice separates from solutions of strengths weaker than the cryohydrate, and (2), the anhydrous salt or some hydrate richer than the cryohydrate, separates from solutions stronger than the cryohydrate. About twenty typical salts have been examined in this manner, and curves were exhibited in which the abscissæ represent strengths, and the ordinates solidification temperatures. All the curves have a similar character and exhibit a point of contrary flexure, between the origin representing pure water at 0° C. and the cryohydrate. Between the cryohydrate and the 0° C. degree of saturation, they are nearly straight lines, and are continuous with the curves of solubility above 0° C. The joint effect of two salts in depressing the temperature of ice-formation was also examined. From previous experiments the general law that the temperature of a freezing mixture is identical with that of the solidification of the cryohydrate of the corresponding salt, appeared not to be the case with iodide of sodium. It now appears that this salt offers no exception to the general law and that what was previously mistaken for the cryohydrate is really a sub-cryohydrate solidifying at a higher temperature. Certain remarkable cases of supersaturation were discussed which show that a solution may be supersaturated in a 3-fold manner, (1) with regard to ice (2) with regard to a salt, and (3) with regard to the cryohydrate of the salt. The parallelism between a boiling saturated salt solution and a glaciating one was pointed out.

PARIS

Academy of Sciences, March 27.—Vice-Admiral Paris in the chair.—The following papers were read:—Influence of variations of pressure on the working of chronometers, by M. Yvon Villarceau.—On the small movements of an incompressible fluid in an elastic tube, by M. Resal. The velocity of propagation of waves is equal to the square root of the product of the coefficient of elasticity and the thickness of the tube divided by that of the diameter of the tube and the density of the liquid.—Observations of temperature at the Museum of Natural History during 1875, with electric thermometers placed in the air and in turf-covered and bare ground, by M. Becquerel. The temperature was, on an average, somewhat higher in the turf-covered ground than in the bare ground, and in the former it never descended below zero.—On the comparative movements of the thermometer and barometer during the commotion of March, 1876, by M. Sainte-Claire-Deville. The periodic oscillation of the temperature from 9th to 13th March did not fail to be produced, and the law of non-synchronic parallelism of temperature and pressure is realised even in the most sudden variations of these two elements.—Remarks *à propos* of Mr. Lockyer's communication on new lines of calcium, by M. Sainte Claire-Deville. In two neighbouring groups of mineral substances, there are most frequently two minerals belonging respectively to each, and characterised by the same basic element. Now of all simple bodies it is calcium that most commonly plays this double *rôle*. Is this connected with its double behaviour under the influence of dissociants?—Experiments on the schistosity of rocks, and the deformations of fossils correlative to this phenomenon; geological consequences of these experiments, by M. Daubrée (first part). The press used in these instructive experiments could give a total pressure of 100,000 kilogrammes on the plates.—Reproduction of Amblystoma observed in the museum, by M. Blanchard. The Amblystoma of Mexico is the adult form of the Axolotl, and the fact observed is important as disproving the idea of the sterility of certain Batrachians in the adult state.—Continued observations of solar protuberances during the second semester of 1875, by P. Secchi. These comprise seven rotations. It is a period of prolonged minimum; the absolute minimum not yet reached (March). Protuberances varying from 2 or 3 one day to 10 or 12 the next. The hydrogenic flames were commonly *straight*, though 2 or even 3 minutes in height (say 60 terrestrial diameters); this indicates great tranquility. The chromosphere was very low at the equator, but often reached a great height at the poles (24 and 30 seconds). This is the effect of displacement of maxima towards the poles.—Mr. Spottiswoode was elected correspondent in the section of Geometry, in room of the late M. Le Besgue.—Report on a memoir of M. Bourgoin, entitled "Researches in the succinic series."—Employment of coal-tar and of sulpho-carbonates against Phylloxera, by M. de la Vergne.—Analytic theory of the movements of Jupiter's satellites, by M. Souillart.—Results of actinometric measurements on the summit of Mont Blanc, by M. Violle.—Velocity of thermal flow in a bar of iron, by M. Decharme. The times taken by the flow to reach different points in the bar are directly proportional to the squares of the distances of these points from the heated end; or, the velocities of thermal flow are inversely proportional to the squares of the distances. (The cooling is slower than the heating).—Study on stratified light; memoir by M. Neyreneuf.—The elephants of Mont Dol; organogeny of the system of molar teeth of the mammoth, by M. Sirodot.—Photomicrographic researches on the transformation of collodion in photographic operations, by M. Girard. Microscopic examination of collodion enables one to know the nature of the layer and to follow the reactions produced in photographic impression.—On communications at a distance by water-courses, by M. Bourbouze (sealed packet deposited in 1870). Lines are dispensed with, and earth currents utilised.—On the conditions of immediate integrability of an expression with ordinary differentials of any order, by M. Pujet.—Impossibility of the equation $x^7 + y^7 + z^7 = 0$, by M. Pepin.—On the exchange of ammonia between natural waters and the atmosphere, by M. Schlœsing. The ammonia condensed by a given quantity of water increases rapidly with diminution of temperature. It is a mistake to suppose the ammonia of a cloud is condensed almost entirely in rain.—Sources of carbonic oxide; new mode of preparation of very concentrated formic acid, by M. Lorin. In this method dehydrated oxalic acid is used in place of sulphuric acid, with a formiate.—On the constitution of the excretory canal of the hermaphrodite organ in *Leucochron candidissima*, Beck, and in *Bulimus decollatus*, Linn., by M. Dubrueil.—On the relations between number of molar teeth in the dog and dimensions of the bones of the face, by M. Toussaint. The normal formula for molar teeth of the dog is $\frac{6}{7}$, but in the extreme, dissimilar types of bulldog and greyhound the formulæ of $\frac{5}{7}$ or $\frac{5}{6}$ for the former, and $\frac{7}{9}$ for the latter, are met with. One may follow the transformation of the formulæ by examining intermediate types.—Researches on the convergence and divergence of formulæ of Fourier's representation, by M. Paul de Bois Reymond.

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